00039804

THE
ASIATIC SOCIETY OF BOMBAY
Town Hall, Bombay.

Digitized with financial assistance from the
Government of Maharashtra
on 18 July, 2018

FREE EVENING LECTURES.

SOUTH KENSINGTON MUSEUM.

REE EVENING LECTURES,

DELIVERED IN CONNECTION WITH

THE

SPECIAL LOAN COLLECTION OF

SCIENTIFIC APPARATUS,

39804
al

cchs2

Published for the Lords of the Committee of Council on Education

BY

CHAPMAN AND HALL, 193, PICCADILLY.

·504
Fæ/Fæ
39804

PRINTED BY TAYLOR AND CO.,
LITTLE QUEEN STREET, LINCOLN'S INN FIELDS.

INTRODUCTION.

DURING the progress of their work it became evident to those who were engaged in organising and arranging the loan collection of scientific apparatus, that its usefulness to the general public would be very much increased, and the interests of science furthered, if explanations of the construction and uses of the various instruments could be given. Many of the exhibitors provided explanations at stated times of the instruments lent by them; but it was not possible to do this for more than a comparatively small proportion of all the apparatus shown, and it was felt that it would be very desirable to have lectures on the classes of instruments and apparatus used for different purposes. In this way information could be given as to the progress in the development of various apparatus, and the respective uses and advantages of different forms of instruments might be explained.

The Science and Art Department was, however, from want of funds, powerless to institute such a course of lectures; but at this stage several scientific men came forward and generously offered their services in giving free lectures on the evenings when the

collection was open to the public. Others whose names are well-known in the scientific world followed their example, and a series of lectures was organised which lasted through the summer and which are printed in this volume.

The Lords of the Committee of Council on Education take this opportunity of tendering their thanks to the gentlemen to whom they and the public are indebted for these lectures, as well as to those who at a later period of the year gave lectures bearing on the same collection, but which it has not been found practicable to publish. That their efforts were highly and widely appreciated by the public was evident from the numbers of the audience and their very attentive and intelligent behaviour, which My Lords trust were some recompense for the trouble and time which the lecturers had so liberally bestowed for the public good.

CONTENTS.

PAGE

INTRODUCTION v

PROFESSOR ROSCOE, F.R.S. On John Dalton's Apparatus, and what he did with it 1

PROFESSOR GUTHRIE, F.R.S. On Cold 25

REV. S. J. PERRY, F.R.S. On The Methods Employed and the Results Obtained in the late Transit of Venus Expedition 39

MR. W. H. PREECE, M.I.C.E., ETC. On Telegraphy . 67

CAPT. ABNEY, R.E., F.R.S. On Photographic Printing Processes 89

DR. SCHUSTER. On The Action of Electric Currents on each other 105

PROFESSOR TYNDALL, F.R.S. On Faraday's Apparatus . 118

THE RIGHT HON. LYON PLAYFAIR, M.P. On Air and Airs, as illustrated by the Magdeburg Hemispheres, and Black's and Cavendish's Balances 134

PROFESSOR GLADSTONE, F.R.S. On Davy's and Faraday's Apparatus 155

PAGE

Rev. R. Main, F.R.S. On Astronomical Instruments. . 173

Professor Francis Guthrie, LL.B. On Heat and Work 198

Dr. Stone. On Modes of Eliciting and Reinforcing Sound 218

Mr. C. V. Walker, F.R.S., F.R.A.S., etc. On Galvanic Time Signals 235

The Earl of Rosse, F.R.S., etc. On Reflecting Telescopes 260

Mr. J. N. Douglass. On The Great and Little Basses Rock Lighthouses 279

Mr. N. Story Maskelyne, M.A., F.R.S., etc. On What is a Crystal? 294

Captain Davis, R.N. On Arctic Discovery in Connection with the Expedition now making its way to the North Pole 317

Professor G. Carey Foster, F.R.S. On Electricity as a Motive Power 344

Captain Davis, R.N. On Antarctic Exploration . . . 366

Professor Herbert M'Leod. On Some Properties of Gases 384

Mr. W. J. Harrison, F.G.S. On Local Geology, with special reference to that of Leicestershire . . . 401

Professor E. Hull, F.R.S. On The Physical Geology of Ireland as compared with that of Great Britain . . 421

Professor Barrett, F.C.S. On The Analogy between Light and Sound 434

Mr. W. Spottiswoode, F.R.S. On The Polarisation of Light 471

Mr. H. W. Chisholm. On Standard Weights and Measures 493

ON JOHN DALTON'S APPARATUS, AND WHAT HE DID WITH IT.

LECTURE BY PROFESSOR ROSCOE, F.R.S.

June 3rd, 1876.

MAJOR DONNELLY, R.E., IN THE CHAIR.

MAJOR DONNELLY, on taking the chair, said Professor Roscoe's reputation among men of Science rendered it quite unnecessary that he should be introduced to the audience, to whom his name was no doubt already quite familiar. But he was glad of the opportunity afforded him of explaining how this series of lectures originated, and pointing out how much they were all indebted to Professor Roscoe, and the other eminent men who had come forward with him to aid in this matter. They were all aware how much the Loan Collection owed to the many men of Science who had devoted so much labour and so much of their valuable time in forming, classifying, and arranging it. Without the help of these able volunteers, the officers of the Science and Art Department would have been quite powerless to cope with the mass of objects of the most interesting kind which had been poured into the Exhibition from all parts of the world. Among the volunteers none had worked harder than Professor Roscoe. And when, after the Collection was opened, it was seen how much it was appreciated by the public, how eagerly any means of informing themselves with regard to the objects were sought after by the visitors, and how much any explanations that were given were valued, Professor Roscoe and a number of other gentlemen volun-

teered to give free evening lectures. This offer was most gratefully accepted. And Major Donnelly said he felt sure that the audience would fully appreciate what was being so liberally done for their instruction and amusement.

He also announced that steps were being taken to give explanations of various objects at stated times during certain days of the week in the galleries.

On the 6th September, 1766, in a poor weaver's cottage, lying on the breezy hills of Cumberland, in the village of Eaglesfield, near Cockermouth, was born a man who was destined to do more for the progress of physical science, and especially of chemistry, than any other with the single exception perhaps of Lavoisier. Sprung from a humble but thrifty north-country Quaker stock, John Dalton, like many of the men who have made their names illustrious in this and in other countries, was self-educated.

His chief mental characteristics were independence of spirit, indomitable determination, and perseverance, clearness and straightforwardness of vision, fearlessness of inquiry, and entirely unselfish and life-long devotion to the advancement of scientific truth. Hear what he says of his own powers. "If I have succeeded better," he writes "than many who surround me, it has been chiefly—nay, I may say, almost solely—from unwearied assiduity. It is not so much from any superior genius that one man possesses over another, but more from attention to study, and perseverance in the objects before them, that some men rise to greater eminence than others." Dalton's early independence and spirit is well shown by the fact related in the interesting life of Dalton, written by my lamented friend Dr. Lonsdale, that one day on the outside of weaver Dalton's cottage, his son John, then about 12 years of age, had posted up a large sheet of white paper, inscribed with a bold hand, containing the announcement of his having opened a school for both sexes on reasonable terms. For a short time he taught this primitive school in an old barn, then in his father's house, and finally in the Friends' meeting-house. His

scholars were of all ages, from infancy to 17; some had to sit on his knees to be taught the alphabet, whilst others threatened their young master, proved highly refractory, and actually challenged him to fight. His Quaker firmness and dogged perseverance stood him in good stead on these occasions, and he came out victorious.

As a schoolmaster he began life, and as a schoolmaster he ended it; and it was by teaching that through life he earned his bread, first in Eaglesfield, then in Kendal, where he undertook a school in conjunction with his brother, and afterwards in Manchester, where, in the year 1793, he was appointed science tutor to the Manchester New College, and lastly as a private tutor of Mathematics and science to anybody who could afford to pay from eighteen pence to half-a-crown per lesson.

This routine occupation of teaching was, however, only the outward occupation of his mind. All the time he was teaching the children at Eaglesfield, and the boys at Kendal, and the young men and young women at Manchester (for even in those days young women learned science), his mind was wholly otherwise engaged, first by solving mathematical problems, which were published in the 'Ladies' Diary,' a magazine in those days much sought after; then by the study in Kendal of many branches of scientific inquiry, especially those of meteorological phenomena, and thus laying the foundation for those great scientific discoveries which have made his name immortal.

The collection of Dalton's apparatus exhibited is only a portion of that which Dalton prepared with his own hands and with which he made his researches. It is interesting and important, not from any inherent value, but because it explains how, with such rude and imperfect appliances, a mind like Dalton's was able to obtain important results. As indicating the slight value which he set upon apparatus of an expensive kind, the following story is told in his life. M. Pelletan, of Paris, visited Manchester in 1820 for the sole purpose of paying his respects to the founder of the atomic theory. He fancied that Dalton would be occupying a professor's chair surrounded by adepts in science and hundreds of ingenuous youths; residing in

a handsome mansion in a handsome square of the city, or enjoying his *otium cum dignitate* in a suburban villa, with roses embellishing its porch; in short, the great representative man of Manchester, and well known and appreciated by every citizen. Judge of his surprise when *Monsieur Dalton, le philosophe,* could only be found after much inquiry, and when found, was engaged looking over the shoulders of a boy figuring numbers on a slate. The Frenchman, doubting his senses, asked the grey-headed gentleman if he really had the honour of addressing *Monsieur Dalton.* "Yes," replied Dalton; "will you sit down till I put this lad right about his arithmetic?" As the stranger gathered confidence he asked Dalton's permission to see his laboratory and philosophical instruments, the employment of which had led to such remarkable discoveries in physics. "Oh!" said Dalton, pointing to a miscellaneous collection of apparatus occupying a corner of the room, "that is all the apparatus I possess."

Dalton's mind was first directed towards the study of meteorology. The constant, reiterated observation thus necessary, and the deductions which could be drawn therefrom, were subjects suited to his peculiar mind. He first began to make general observations on the weather, and then to record in tabulated form the indications of the barometer, thermometer, and hygroscope, all of his own construction, for in those days philosophical apparatus in the north of England was difficult to procure, and even could it have been met with, his means did not enable him to purchase. "The barometer," he says, "is graduated into $\frac{1}{10}$ of an inch, the thermometer (on Fahrenheit's scale) is a mercurial one exposed to the open air, but free from the sun." The hygroscope was made from about 6 yards of whip-cord suspended from a nail, with a small weight to stretch it; its scale of inches began from no certain point, the less the number the shorter the string and the greater the moisture.

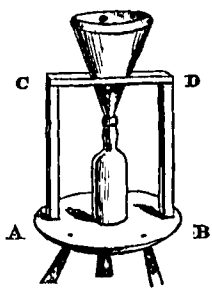
On March the 24th, 1787, he began a regular record of meteorological phenomena, and this record he continued with uninterrupted sequence to the last day of his life, on the 27th of July, 1844. The rain-gauge, which he describes, and of which I have a model before me, was characteristic from its simplicity.

The letter in which he describes this rain-gauge to a Miss Hudson, one of his Eaglesfield pupils, is interesting, and shows that in those days the young women of the Cumberland villages had sufficient education to understand decimals, and the use of a scientific instrument:

"*Kendal*, 8 *mo.*, 4*th*, 1788.

"Respected Friend,—The study of Nature having been with me a predominant inclination, it is not unlikely that I should be ready to prompt others to the same. I have been tempted to think that thou would take a pleasure in remarking the quantity of rain that falls with you each day, if thou knew with what facility the same is effected. I have observed here that people who are entirely ignorant of the matter suppose it a work of great labour and difficulty, and which can only be done by those they call great scholars. This, however, is a mistake. A very little knowledge of mensuration is sufficient for the theory of it, and nothing but plain addition is wanted in the practice.

"The annexed scheme will represent the most simple apparatus: A.B. is a three-foot stool, to be fixed in a garden bed, etc. A.C and B D two posts fixed in the same about 11 or 12 inches, and support the arm CD, which is 1½ inch broad and 1 deep; the pipe of the funnel exactly fits the hole in CD, keeping the funnel firm and level. The funnel may be 6, 7, or more inches over; and if it have an upright rim of an inch, it is better, but will do without it. Also, it should be painted to save it from the weather. A common glass bottle will hold all the water that falls at any time in 24 hours, if the funnel be only 6 or 7 inches in diameter; except, perhaps, two or three days in the year. A pair of scales, with a few small weights, are requisite.



"Now, to determine the depth of water that falls on any level surface from the above, we have the following tables made for funnels of 6 and 7 inches, wherein are set down the depths, corresponding to the several weights, in decimal fractions. And

any person who has learned mensuration will be able to adapt a table to any funnel, by knowing that 62½ lbs. avoirdupois equal 1 cubic foot of water :—

Suppose there is caught with a funnel of 6 inches diameter 1 lb. 3 oz. 5¼ drs. of water, required the depth.

1 lb.= ·9778
2 oz.= ·1222
1 = ·0611
4 drs.= ·0153
1 = ·0038
¼ = ·0010

1·1812

Weights.	Diameters of Funnels.	
lb. av.	6 inches.	7 inches.
1	·9778	·7184
oz.		
8	·4889	·3592
4	·2445	·1796
2	·1222	·0898
1		·0449
drs.	·0611	
8	·0306	·0225
4	·0153	·0112
2	·0076	·0056
1	·0038	·0028
½	·0019	·0014
¼	·0010	·0007
⅛	·0005	·0004

"That is, the depth that would have fallen on a level surface will be 1 inch, 1 tenth, 8 hundred, 1 thousand, and 2 ten thousand parts of an inch.

"Suppose with a funnel of 7 inches there is caught 1 oz. 7½ drs.

1 oz. =·0449
4 drs.=·0112
2 ·=·0056
1 ·=·0028
½ ·=·0014

·0659

That is, 6 hundredth, 5 ten hundredth or thousandth, 9 ten thousandth part of an inch.

"N.B.—The water is supposed to be taken at stated hours, as 6, or 8, or 10 at night.

"By this time I apprehend the difficulty generally supposed to attend this matter is removed. I should be glad if thou, or any in your neighbourhood, on whose accuracy one might rely, would find it agreeable and convenient to notice this matter; but, however, I do not mean to request it, but only to show the easiness with which it's done. Ignorance, no doubt, will look upon this as a trifling and childish amusement, but few of this nature are such in a philosophical sense. If to be able to predict the state of the weather, with tolerable precision, by which great advantage might accrue to the husbandman, to the mariner, and to mankind in general, be at all an object worthy of pursuit, that person who has in any way contributed to attain it cannot be said to have lived or to have laboured in vain. I am, respectfully, thy friend,

"JOHN DALTON."

"To Sarah Hudson, Eaglesfield."

His thermometers, several of which are in the exhibition, were as a rule all made by himself.

The following letter to his cousin Elihu Robinson, to whom in his boyhood he had owed much, gives a description of the method which he employed for making his thermometers:

"*Kendal*, 8 mo., 23rd, 1788.

"Dear Cousin,—Herewith thou wilt receive, I hope safely, two thermometers with somewhat longer scales than the former; please to take thy choice of the three, to let John Fletcher have the next choice, and to reserve the other till my brother comes.

"You will probably choose by the length of the scales; but those with the least bulbs will soonest come to the temperature of the surrounding medium. However, the largest, I apprehend, will rise or fall to within a degree of the proper place in half an hour in the air. Thou may try whether that thou hast already is with these two or not, by dipping the bulbs into a basin of water for five minutes.

"Possibly the manner of making them may not be unentertaining. A small receptacle being fixed on the end of the tube, a quantity of mercury is poured into it, part of which runs down the tube so as to half fill the bulb, and then stops, the tube being still

filled with mercury, which is unable to fall by reason of the pressure of the air in the bulb. Then a candle is applied to the bulb, which, rarefying the air contained in it, raises the mercury in the tube quickly to the top, and then it escapes in bubbles through the mercury in the receptacle. This done, it is cooled again, when the internal air contracting, another portion of mercury falls down into the bulb; and this operation is repeated till all the air is expelled. Then the mercury is heated above boiling water, and the end of the tube melted and closed at the same time, when, the mercury subsiding, there is left a vacuum; this is done chiefly to keep the moisture, dust, etc., out of the tube. The whole is then put into boiling water, when the barometer stands at 30 inches, and the boiling point thereby determined; afterwards (if circumstances admit), the freezing point is found by putting it into a mixture of water and pounded ice, or water and snow, which, when melting before the fire, keep at an invariable point (32°) till the whole is melted. If this cannot be done, as in summer, it may be set by another thermometer, and the scale adapted accordingly. N.B.—As the freezing points of these two were not found on account of the season, it will not be amiss to try whether they are accurate, when a convenient season comes.

"The principle on which they act needs little explication; as mercury, like most other bodies, is subject to be contracted by cold and expanded by heat; and as the capacity of the bulb remains always filled, the total variation of the mercury in bulk, it is evident, will be manifested in the tube.

"The range of the thermometer is little in these parts compared with the northern. At Petersburgh the summer heat is equal to ours, but in winter severe cold predominates; the thermometer is frequently found 40 or 60 below nothing; and in Siberia it has been observed, even 100 or 120 below nothing. On the contrary, in the burning sands of Africa it reaches 120 or 140 above nothing. Is not the internal principle of heat in man and other animals a wonderful phenomenon, that can sustain these two extremes without any sensible variation?

"Remark.—Réaumur's scale (used by the French and others)

counts from 0 at the freezing point to 80° at the boiling point; consequently 2¼ degrees Fahrenheit are equal to 1 of Réaumur.

"Abstract of my journal for the present year.

Thermometer without.				Rain. Inches and decimals.	Wet days.	Auroræ Boreales.
	mean	highest	lowest			
1 mo.	39°	47	20	5·6160	29	6
2 mo.	38·3	47	28	3·3064	23	2
3 mo.	36·8	50	18	2·1883	16	4
4 mo.	46·3	69	32	2·9047	16	11
5 mo.	53°	80	38	1·1872	10	7
6 mo.	57·3	80	45	2·3137	7	2
7 mo.	56·8	68	47	7·0323	28	1

Thunder-storms.

5 mo., 19. 2 P.M., distant, W.
„ 26. 7 P.M., frequent loud peals, very near.
7 mo., 3. 6 P.M., frequent peals, some very near.
8 mo., 16. 7½ P.M., distant about 8 miles, S.E., but loud and tremendous; about 20 or 30 flashes were observed in as many minutes, and the reports of each heard though the cloud was but just visible above the horizon; the zenith clear. My love to cousin Ruth, self, and family.

"JOHN DALTON."

In a letter of May 24, 1788, written to his friend Mr. Crosthwaite, of Keswick, Dalton describes minutely the mode he adopted for constructing a barometer, which he sold for the modest sum of 18 shillings. In these early days he omitted to boil or even to heat the mercury after he had placed it in the tube, so that both air and moisture were present. The error arising from this cause he appears to have discovered soon afterwards, for he writes:—"I intend to renew mine as soon as convenient; if thou do the same, be careful in undoing it, and attend to the cautions I give. Be sure to rub the inside of the tube well with

warm dry cotton or wool; and have the mercury, when poured in, at least milk-warm, for moisture is above all things else to be avoided, as it depresses the mercury far more than a particle of air does: mine is, as I have said, at least $\frac{1}{16}$th of an inch too low, and yet it is clear of air, and to all appearances dry; but I doubt not but attending to these precautions, which I knew nothing of when it was filled, will raise it up to its proper height."

The results of observations made with these instruments were embodied in a volume of meteorological essays published in London in the year 1793, in which he discusses amongst other subjects the fluctuations of the barometer and the phenomena of the aurora borealis.

In the year 1793 Dalton removed from Kendal to Manchester, and he was 30 years of age before he gave any special attention to chemistry. The first communication on a physical subject which Dalton made to the Philosophical Society in Manchester was on his own peculiarity of colour-blindness, read in October, 1794; and his second, read on March 1st, 1799, was an important one as marking the commencement of his attachment to physical research. The question of the observations on the rain-fall had often led him to consider the question of how the moisture is taken up by the air, and he then came to the following conclusions:

(1.) That aqueous vapour is an elastic fluid diffusible in the atmosphere, but forming no chemical combination with it; (2) that the maximum of vapour contained in the atmosphere is regulated by the temperature alone; and (3) that throughout the atmosphere a quantity of vapour is always contained, varying in amount according to circumstances.

To show the use that he made of his thermometers, as well as to indicate his accurate power of observation, the following extract from a letter to his brother, written in 1799, is of interest:—"I have lately been making some curious experiments on the congelation of water in certain circumstances. I have cooled it down to 5° or 6° (Fahrenheit) without freezing, by putting it into a thermometer tube. I find it also impracticable to

freeze it in such circumstances above 15° or 20°; when it does freeze it is instantaneous, and the liquor shoots up the tube as if ejected by a syringe, and often bursts the tube with a report."

He next determined the point of maximum density of water, finding it to lie between 36° and 38°, whereas we now know from the accurate experiments of Joule, that the point lies at 39°.101.

In October, 1801, Dalton read several memoirs before the Manchester Society: (1) Experimental essays on the constitution of mixed gases; (2) on the force of steam or vapour from water and other liquids in different temperatures, both in a torricellian vacuum and in air; (3) on evaporation; and (4) on the expansion of gases by heat. Dalton's law of evaporation may be stated as follows, in the words of Professor Balfour Stewart:—"In a space destitute of air, the vaporisation of a liquid goes on until the vapour has attained a determinate pressure, dependent on the temperature, so that in every space void of air which is saturated with vapour, determinate pressure corresponds to determinate temperature." When gas and vapour are mixed together in a confined space, the law discovered by Dalton may be expressed as follows:—"In a space filled with air the same amount of water evaporates as in a space destitute of air; and precisely the same relation subsists between the temperature and the pressure of the vapour whether the space contains air or not." It has now been found by more accurate experiments that the law of Dalton is not quite correct, Regnault having shown that the tension in air is always about 2 per cent. less than in vacuo. Nevertheless, Regnault is inclined to believe that Dalton's view is the true one, and that the difference observed may be due to the absorbing influence of the walls of the containing vessel.

Dalton's second essay, "On the Force of Steam or Vapour from Water and other Liquids at different Temperatures," is one of great importance, as furnishing a means of determining the absolute quantity of moisture contained in a given volume of air, after the dew-point has been ascertained. In this essay too, we find the remarkable fact that Dalton anticipated by several years the discovery made by Northmore and Faraday, of the condensation

of gases. "There can scarcely be a doubt entertained," says Dalton, "respecting the reducibility of all elastic fluids, of whatever kind, into liquids; and we ought not to despair of effecting it in low temperatures, and by strong pressure exerted upon the unmixed gases." Although this conclusion has not been universally carried out, yet the expression of the possibility of condensing one form of matter into another is one which exhibits Dalton's power of scientific insight.

In the exhibition we have an instrument, No. 17, used by Dalton for the determination of the tension of the vapour of ether, and this is interesting as being the means by which Dalton arrived at one of his most important experimental laws, viz., "That the variation of the force of vapour from all liquids is the same for the same variation of temperature, reckoning from vapour of any given force."

He describes the instrument as follows: "The ether I used boils in the open air at 102°. I filled a barometer tube with mercury moistened by agitation in ether; after a few minutes a portion of the ether rose to the top of the mercurial column, and the height of the column became stationary. When the whole had acquired the temperature of the room (62°), the mercury stood at 17·00 inches, the barometer being at the time 29·75 inches. Hence the force from the vapour of ether at 62° is equal to 12·75 of aqueous-vapour at 172°, which are 40° from the respective boiling-points of the liquids."

The apparatus which it is believed Dalton employed for the measurement of the expansion of gases by heat is exhibited in Nos. 35 and 36. They consist of bulb tubes with graduated scales. Dalton ascertained by repeated experiments that 1000 volumes of common air of the temperature of 55°, and common pressure, expand to 1325 volumes when heated to a temperature of 212°, and he concluded that any gas at any temperature increases in volume for a rise of one degree by a constant fraction of its bulk at that temperature. Other gases too he found expand equally with air; thus hydrogen, oxygen, carbonic acid, and nitrous gas, were observed by him, the small differences he noticed being due to the presence of aqueous vapour. Hence Dalton concluded "that all elastic fluids, under the same pressure, expand equally by heat."

These researches, as well as the instruments which are here exhibited, indicated the character of Dalton's genius. His apparatus, rudely and roughly made with his own hands, were incapable of affording correct results, and his manner of experimentation was the reverse of accurate. In spite, however, of the faultiness of his apparatus, his experiments were not only skilfully devised, but, as Dr. Henry remarks, they were most skilfully interpreted, so that the result of the combination of experiment and deduction was the discovery of general laws of the relation of vapour to air which still constitute the foundation of meteorological science.

The apparatus marked Nos. 21, 22, and 23, in the catalogue, indicates a new point of departure of Dalton's experimental investigations, for these rough instruments are the eudiometers with which he made his first chemical analysis; namely, the determination of the composition of the air. On November the 12th, 1802, Dalton communicated to the Manchester Society the results of an "experimental enquiry into the proportion of the several gases or elastic fluids constituting the atmosphere."

These he ascertained by weight to be as follows:—

Azotic gas	75·55
Oxygenous gas	23·32
Aqueous vapour	1·03
Carbonic acid gas	0·10
	100·00

This same paper possesses, however, a still greater interest, as it is the one in which for the first time the existence of chemical combination in multiple proportions is clearly stated. He tells us that the oxygen contained in 100 volumes of common air will combine in a narrow tube with 36 volumes of pure nitrous gas, forming nitric acid, or with 72 in a wide vessel to form nitrous acid. "These facts," he states, "clearly point out the theory of the process: the elements of oxygen may combine with a certain proportion of nitrous gas, or with twice that portion, but with no intermediate quantity."

Here again we have an example of the clearness of perception

which enabled Dalton to argue correctly from inexact experiments. If we repeat the experiment as described, we do not obtain the results he arrived at. Oxygen cannot, as a fact, be made to combine with nitric oxide in the proportion of 1 to 2 by, according to the plan proposed by Dalton, merely varying the shape of the containing vessel, although by other means we can now effect these two acts of combination. Hence, we see that Dalton's conclusions were correct, although it appears to have been a mere chance that his experimental results rendered such a conclusion possible.

In January, 1803, he opened a new mine of investigation in a paper "On the tendency of elastic fluids to diffusion through each other;" and here again we have to notice the extreme simplicity of the means he adopted to obtain important results. He took two phials connected together by a glass tube 10 inches long and $\frac{1}{20}$ of an inch in diameter; the upper phial he filled with a light gas (hydrogen), the lower with a heavy gas (carbonic acid). After the lapse of some time he found that the gases in the phials thus connected together had uniformly diffused; that the heavy gas had ascended, whilst the lighter gas had descended, and thus he came to the conclusion that elastic fluids of different specific gravities do not separate after long standing, but remain in a condition of uniform and equal diffusion. In the collection next to Dalton's instruments stands the apparatus of Thomas Graham, and this is as it should be, for Graham worked up the rough block which had been prepared for him by Dalton, determining the rates of diffusion of different gases, and finding these to be inversely proportional to the square roots of their densities.

The period with which we are now engaged was certainly Dalton's most prolific one. A paper, "On the absorption of gases by water and other liquids," read on October 21st, 1803, is important, because it contains the first germs of his celebrated Atomic Theory. In considering the mode in which gas is dissolved in water, Dalton states it as his opinion that the particles of gas are dissolved in water much in the way that particles of sand can be mixed with a quantity of small shot, and he concludes that the fact of water not dissolving all gases in the same quantity depends

"upon the weight and number of the ultimate particles of the several gases, those whose particles are lightest and single being least absorbable, and the others more, according as they increase in weight and complexity." He continues, "An enquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new. I have lately been prosecuting this enquiry with remarkable success." Then follows a table (printed in 1805), which is interesting as being the one in which the relative weights of the ultimate particles of gaseous and other bodies is for the first time given.

FIRST TABLE OF THE RELATIVE WEIGHTS OF THE ULTIMATE PARTICLES OF GASEOUS AND OTHER BODIES (JOHN DALTON, 1803):—

Hydrogen	1
Azot	4·2
Carbon	4 3
Ammonia	5·2
Oxygen	5·5
Water	6·5
Phosphorus	7·2
Phosphuretted hydrogen	8·2
Nitrous gas	9·7*
Ether	9·6
Gaseous oxide of carbon	9·8
Nitrous oxide	13·9*
Sulphur	14·4
Nitric acid	15·2
Sulphuretted hydrogen	15·4
Carbonic acid	15·3
Alcohol	15·1
Sulphureous acid	19·9
Sulphuric acid	25·4
Carburetted hydrogen from stagnant water	6·3
Olefiant gas	5·3

* Misprints of 9·3 and 13·7 occur here in the original table.

A glance at this table shows us that Dalton took hydrogen, being the lightest substance known, as the unit of comparison, and he compared the weights of the ultimate particles of all the other elements and compounds with that of hydrogen taken as 1. Then he found that the atom or ultimate particle of azot, or nitrogen as we now call it, was 4·2; that of carbon 4·3, that of oxygen 5·5, and so on. A more careful inspection of the table, especially with regard to the two gaseous oxides of nitrogen and of carbon named in the list, will reveal to us Dalton's views as to the constitution of these substances. Thus opposite nitrous gas—now termed nitric oxide gas—we find the figures 9·7. What do these signify? They mean that this gas is made up of one atom of nitrogen (or azot) weighing 4·2, and one atom of oxygen weighing 5·5, and that the weight of the compound atom (if we may use the term) of nitrous gas weighs 9·7. Opposite nitrous oxide we find placed 13·9; this means that the ultimate particle of this gas contains two atoms of azot weighing twice 4·2, and one atom of oxygen weighing 5·5. In like manner the gaseous oxide of carbon has the number 9·8 placed against it, viz., $4{\cdot}3 + 5{\cdot}5$; whilst opposite carbonic acid we find 15·3, viz., $4{\cdot}3 + 2 \times 5{\cdot}5$.

The interesting question now arises, how did Dalton obtain these numbers? Upon what experimental basis does the determination of these first chemical constants rest?

In the second part of his New System of Chemical Philosophy, published in 1810, Dalton points out under the description of each substance the experimental evidence upon which its composition is based, and explains, in some cases, how he arrived at the relative weights of the ultimate particles in question. In the five previous years, however, considerable changes had been made by Dalton in the numbers; the table found in the New System being not only much more extended, but the numbers differing in many cases altogether from those found in the above table published in 1805. It is therefore, unfortunately, to a considerable extent now a matter of conjecture how Dalton arrived at the first set of numbers. All we know is that it was mainly by the consideration of the composition of certain simple gaseous com-

pounds of the elements that he arrived at his conclusions, and in order that we may form some idea of the data he employed we must make use of the knowledge which chemists at that time (1803–5) possessed concerning the composition of the more simple compound gases.

The first point to ascertain, if possible, is how Dalton arrived at the relation between the atomic weights of hydrogen and oxygen given in the table as 1 to 5·5 (but altered to 1 to 7 in 1808). The composition of water by weight had been ascertained by the experiments of Cavendish and Lavoisier to be represented by the numbers 15 of hydrogen to 85 of oxygen, and the result was generally accepted by chemists at the time, amongst others doubtless by Dalton. That in those early days Dalton had actually repeated or confirmed these experiments appears improbable. At any rate he formed the opinion that water was what he called a binary compound, *i.e.*, that it is made up of one atom of oxygen and one atom of hydrogen combined together. Hence if he took the numbers 85 to 15 as giving the composition of water, the relation of Hydrogen=1 to Oxygen would be as 1 to 5·6, or nearly that which he adopted. It does not appear possible to explain why Dalton adopted 5·5 instead of 5·6 for oxygen; it may perhaps have been a mistake or a misprint, as there are two evident mistakes in the table.

Let us next endeavour to ascertain how he obtained the number 4·3 for carbon (altered to 5 in 1808 and 5·4 later on). Lavoisier, in the autumn of 1783, had ascertained the composition of carbonic acid gas by heating a given weight of carbon with oxide of lead, and he came to the conclusion that the gas contained 28 parts by weight of carbon to 72 parts by weight of oxygen. Now Dalton was not only acquainted with the properties and composition of carbonic acid, but he was aware that Cruikshank had shown in 1800 that the only other known compound of carbon and oxygen, carbonic oxide gas, yields its own bulk of carbonic acid when mixed with oxygen and burnt; and also that Desormes analysed both these gases, finding carbonic oxide to contain 44 of carbon to 56 of oxygen, whilst carbonic acid contained to 44 of carbon 112 of oxygen, being just double of that in the carbonic

oxide. Dalton adds, "this most striking circumstance seems to have wholly escaped their notice." Hence Dalton assumed that one atom of carbon is united in the case of carbonic oxide with one atom of oxygen, whilst carbonic acid possessed the more complicated composition and contains two atoms of oxygen to one of carbon. Now if carbonic acid contains carbon and oxygen in the proportion of 28 to 72, carbonic oxide must contain half as much oxygen, viz., 28 of carbon to 36 of oxygen, and assuming that the atomic weight of oxygen is 5·5 that of carbon must be $\frac{28 \times 5{\cdot}5}{36} = 4{\cdot}3$. Having thus arrived at the number 4·3 as the first atomic weight of carbon, it is easy to see why Dalton gave 6·3 as the atomic weight of carburetted hydrogen from stagnant water, and 5·3 as that of olefiant gas. The one represents 1 atom of carbon to 2 of hydrogen, the other 1 of carbon to 1 of hydrogen, or olefiant gas contains two equal quantities of carbon, only half as much hydrogen as marsh gas. This conclusion doubtless expressed the results of Dalton's own experiments upon these two gases which were made, as we know from himself, in the year 1804. He proved that neither of these gases contains anything besides carbon and hydrogen, and ascertained—by exploding with oxygen in a Volta's Eudiometer—that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does, and that "just half of the oxygen expended on its combustion was applied to the hydrogen and the other half to the charcoal. This leading fact afforded a clue to its constitution." Whereas, in the case of olefiant gas, two parts of oxygen are spent upon the charcoal and one part upon the hydrogen.

The atomic weight of nitrogen (azote=4·2) was doubtless obtained from the consideration of the composition of ammonia, whose atomic weight is given in the table at 5·2. Ammonia was discovered in 1774 by Priestley, but the composition was ascertained by Berthollet in 1775, by splitting it into its constituent elements by means of electricity, when he came to the conclusion

that it contained 0·193 parts by weight of hydrogen to 0·807 parts by weight of nitrogen. Dalton assumed that this substance is a compound of one atom of hydrogen with one of nitrogen, and hence he obtained for the atomic weight of azote $\frac{807 \times 1}{193} = 4{\cdot}2$; and $4{\cdot}2 + 1 = 5{\cdot}2$ as the atomic weight of ammonia. It is also probable that Dalton made use of the composition of the oxides of nitrogen for the purpose of obtaining the atomic weight of nitrogen. If we take the numbers obtained partly by Davy and partly by himself, as given on page 318 of the New System, as representing the composition of the three lowest oxides, it appears that the mean value for nitrogen is 4·3 when oxygen is taken as 5·5. In all probability the number in this table (4·2) was obtained from an experiment of Dalton's made at an earlier date.

It is not possible to ascertain the exact grounds upon which Dalton gave the number 7·2 for phosphorus; its juxtaposition, however, in the table to phosphuretted hydrogen shows that it was probably an analysis or a density determination of this gas which led him to the atomic weight 7·2, under the supposition that this gas (like ammonia) consisted of one atom of each of its components. In the second table, published in 1808, Dalton gives the number 9 as that of the relative weight of the phosphorus atom, and we are able to trace the origin of this latter number, although that of 7·2 is lost to us. On p. 460, Part II. of his New System, Dalton states that he found 100 cubic inches of phosphuretted hydrogen to weigh 26 grains, the same bulk of hydrogen weighing 2·5 grains; hence, assuming that equal volumes contain an equal number of atoms, we have: $\frac{26 - 2{\cdot}5}{2{\cdot}5} = 9{\cdot}4$ gives the atomic weight of phosphorus nearly. It was probably by similar reasoning from a still more inaccurate experiment than this one that he obtained the number 7·2.

Sulphur, which stands in the first table of 1803 at 14·4, was altered in the list published in the New System to 13. These numbers were derived from a consideration (1) of the composition of sulphuretted hydrogen, which he regarded as a compound of one atom of sulphur with one of hydrogen, and (2) of that of

sulphurous acid, which he supposed to contain one atom of sulphur to two of oxygen. Dalton knew that the first of these compounds contained its own volume of hydrogen, and he determined its specific gravity, so that by deducting from the weight of one volume of the gas that of one volume of hydrogen he would obtain the weight of the atom of sulphur compared to hydrogen as the unit. The specific gravity he obtained was about 1·23 (corresponding nearly he says—p. 451—to Thénard's number 1·23; hence (as he believed air to be 12 times as heavy as hydrogen) he would obtain the atomic weight of sulphur as $(12 \times 1{\cdot}23) - 1 = 13{\cdot}76$, which number, standing half way between 14·4 as given in the first table and 13 as given in the second, points out the origin of the first relative weight of the ultimate particle of sulphur. So from sulphurous acid he would obtain a similar number, taking the specific gravity as obtained by him (Part II. 389) to be 2·3, and remembering that this gas contains its own bulk of oxygen (p. 391), he obtaiued $(2{\cdot}3 - 1{\cdot}12) \times 12 = 14{\cdot}16$ for the atomic weight of sulphur. As however we do not possess the exact numbers of his specific gravity determinations, and as we do not exactly know what number he took at the time as representing the relations between the densities of air and hydrogen (in 1803 he says that the relation of 1 : 0·077 is not correct, and that $\frac{1}{13}$ is nearer the truth), it is impossible to obtain the exact numbers for sulphur as given in the first table.

Thus we see that Dalton arrived at the conclusion that chemical elements can only combine in certain proportions or in multiples of those simple proportions, as he himself informs us from the examination in the year 1804 of the compounds which we now know as carbonic oxide and carbonic acid, and of marsh gas and olefiant gas. Thus he proved that carbonic acid gas contains exactly twice as much oxygen as carbonic oxide contains, finding it impossible to get any intermediate compounds containing less than double the quantity of oxygen. Thus, too, marsh gas, or fire damp, and olefiant gas both contain carbon and hydrogen, but the olefiant gas contains, as Dalton showed, exactly twice as much carbon as marsh gas does; the marsh gas burns with a scarcely luminous flame, but the flame of olefiant gas is much more

luminous, proving that it contains more carbon than the marsh gas.

John Dalton was not satisfied with the mere statement of the fact of chemical combination in multiple proportion; his mind was of an essentially mechanical turn, and he desired to be able to explain these facts. He wished to know *why* this was so. He wished to know why it was that one compound contained just twice as much of one constituent as the other compound did, and why we cannot produce a substance containing an intermediate quantity of the constituent. Pondering on this subject, he arrived at his atomic theory, which serves to explain the facts of the law of "*multiple proportions.*"

Dalton said, if we suppose that atoms exist, and if by their contact they form chemical compounds, and further supposing that the atom of each elementary body has a fixed weight which differs from that of the atom of any other element, then we are able to explain why we get combinations to occur only in this proportion of one given weight, and twice that weight, or three times that weight, and so on. It is of interest here to remember that a great living physicist, Sir William Thomson, has calculated approximately — for we are as yet unable to do more—how large, or rather how small, the atoms are; and he has come to this conclusion—that if you were to take a drop of water, and magnify it up to a globe of the size of the earth, then the atoms contained in that drop of water would not be so large as cricket balls, nor so small as shot pellets. This may serve to give an idea of the minute character of the atoms of which matter is composed.

Now let us attempt to get hold of the idea present in John Dalton's mind. He argued thus: All the atoms of nitrogen have each the same definite unalterable weight, whilst all the atoms of oxygen weigh the same, but the weight of the oxygen atom is different from that of the nitrogen atom. What happens, he then continues, when this atom of nitrogen combines with an atom of oxygen? When they come into contact and clash together a chemical compound is formed, a substance differing altogether both from nitrogen and from oxygen—a substance

having peculiar properties of its own—a distinct chemical compound. Now, he continues, if any chemical action takes place between oxygen and nitrogen, the least quantity of each which can combine is one atom, because the atom is indivisible, and, if more nitrogen is capable of entering into combination with the substance already formed, to produce a second new substance, the smallest quantity of nitrogen which can do this is again one atom; so that, of the two compounds, one consists of one atom of oxygen and one atom of nitrogen, and the other of one atom of oxygen and two atoms of nitrogen.

Thus: $\boxed{N} + \boxed{O} = \boxed{NO}$

And $\boxed{N}\,\boxed{N} + \boxed{O} = \boxed{NNO}$

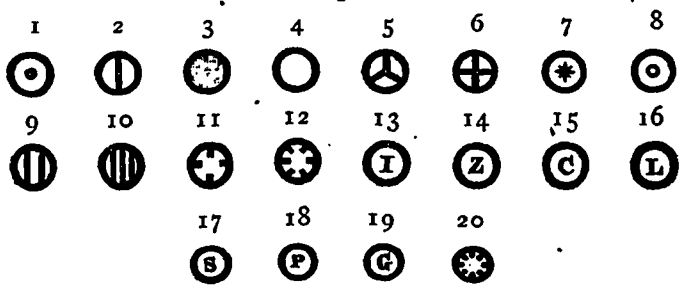
We may build up these compounds with our cubes: here is the one, and here is the other. It is now clear why we can produce no compounds intermediate between these two—we cannot divide the atom of nitrogen. It is an indivisible particle, and is, therefore, the smallest portion of nitrogen capable of entering into combination.

Thus, then, Dalton built up his atomic theory; which differed essentially from the atomic theories of his predecessors, inasmuch as he for the first time introduced the idea of weight, assuming that the atoms of the different elements possess different weights. In order to make this theory more manifest, Dalton was in the habit of drawing his atoms, for he had a strictly mechanical turn of mind. Here you see the mode in which Dalton pictured or symbolised his atoms. This is a drawing of Dalton's atoms, expressed by symbols: hydrogen (No. 1) is represented by a circle with a dot in the middle, oxygen (No. 4) by a simple circle, carbon by a shaded circle (No. 3), nitrogen by one divided in half, and the other elements were expressed by circles with a line or a cross drawn through or upon them. Compounds are represented by the juxtaposition of the component elements:—thus, 21 represents water, 26 nitrous oxide, 31 sulphuric acid, and 37 sugar.

This will give you an idea of the matter-of-fact as well as speculative character of Dalton's mind, and how he made clear to

Elements.

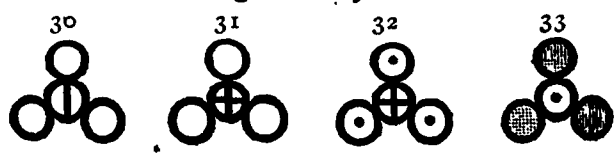
Simple.



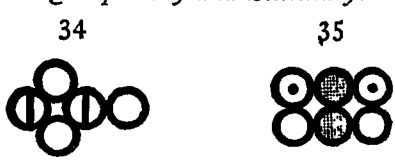
Binary.

Ternary.

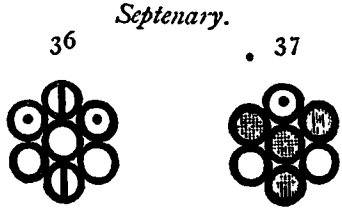
Quarternary.



Quinquenary and Sextenary.



Septenary.



himself and to the world the new notion of the existence of these elementary atoms, each one having a given unalterable weight by which the element was characterised.

As might be expected, from seeing the rough weights made with his own hands, the rough balances, and his inexactly graduated measures which are exhibited, Dalton's numerical results did not stand the test of time; indeed, he subsequently corrected his numbers himself, and in many cases expressed a doubt as to their accuracy. Many of his numerical results have had to be re-modelled, but, although his details have thus had to be changed, the principles on which Dalton founded his theory itself remain firmly fixed; every subsequent discovery and every subsequent investigation having only served to confirm and corroborate the truth of his conclusions and the value of his labours.

To Dalton, then, may fairly be ascribed a position second in the ranks of chemists; second only to that of Lavoisier. It was he who first gave precision to our science, inasmuch as before his time the calculation of chemical quantities had been an impossibility, and the atomic theory remains the key-stone of the arch upon which the science of chemistry rests.

The lesson which I should wish to impress upon your minds whilst you look at these simple remains of departed greatness placed, as they are, amongst the complicated and beautiful examples of modern scientific apparatus, is this: that, after all, the greatest results of science have been obtained, and perhaps may even yet be obtained, by simple means. That without Dalton's persevering devotion, the best and most costly apparatus is useless, whilst earnestness and determination may accomplish much with tools even as rude as those employed by the great Manchester chemist.

The CHAIRMAN conveyed the best thanks of the meeting to Professor Roscoe for his Address.

COLD.

By Frederick Guthrie.

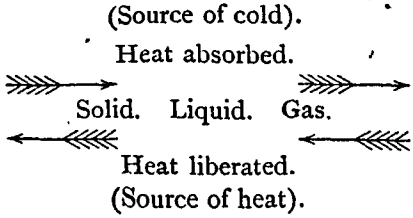
Saturday, June 10th.

The Chair was taken at 8 o'clock by Macleod of Macleod.

The Chairman: Last Saturday was the first of these addresses, and I then intimated to you that the Lords of the Committee of Council on Education had been very glad indeed to avail themselves of the kindness of gentlemen of scientific pursuits to give addresses to you on the subject of the instruments which are collected in this Exhibition. For the second of these lectures I have the pleasure of introducing to you Professor Guthrie, who will address you on the subject of the Production of Cold.

Frederick Guthrie: Mr. Chairman, Ladies and Gentlemen,—It behoves a scientific man, beyond all others, perhaps, to be precise in his language, and this precision is never more necessary than when one is talking about heat and cold. For instance, if I say that a body gets hot as it gets cold, of course I speak a palpable untruth; but if I say a body gets heat as it gets cold, I not only speak a truth, but I speak the one which I wish to develope this evening.

In starting, I can do no better than refer at once to this little diagram, which furnishes the key to all I have to say.



I refer to the three forms of matter, as they are called, solid,

liquid, and gaseous; and here I make no distinction whatever between a gas and a vapour. Accordingly, if we have one and the same body, such as ice, and melt that, it becomes water; boil the water, it becomes steam. One and the same chemical substance assumes these three forms. For the melting of ice you have to put heat into the ice, for the boiling of water you have to put heat into the water; in other words, the solid absorbs heat in order to become a liquid, and the liquid absorbs heat in order to become a gas or vapour. And when the solid is absorbing heat, it gets that heat from whatever it can, and it thus becomes a source of cold; when a liquid passes into the state of a vapour it requires heat to do so, and it thus becomes a source of cold. And here, again, let me return to the ambiguity of language; just as an avaricious man may be a source of poverty to his neighbour, seeking and grasping money, so a body seeking and grasping heat is a source of cold to neighbouring bodies. The very fact that a body is getting heat shows that other bodies, from which it gets that heat, are losing heat. Again, in order to convert a gas into a liquid you must deprive it of heat. You take the heat out of steam in order to condense it. In order to convert a liquid into a solid you must take the heat out of the liquid, or if by any means the gas becomes a liquid, then it gives up heat; it no longer requires so much heat, and the difference it gives up. It becomes, therefore, a source of heat in the act of giving that heat up. So when a liquid becomes a solid, in the act of solidification it gives up heat; in giving this heat up it becomes a source of heat.

We may, therefore, in contemplating this diagram, regard it from two points of view. We may look upon ourselves as the active agent who puts heat into the solid, converting it into a liquid; or into the liquid, converting it into a gas; taking heat out of the gas and converting it into a liquid, or taking heat out of the liquid and converting it into a solid. Or we may look upon ourselves as the observers, and note how the heat is concerned when these changes take place by any other means. The chapter of this long book of heat, which I wish to discuss this evening, is the Source of Cold. That is, when either a solid passes into the

liquid state or when a liquid passes into the gaseous state, not by the addition of heat effected by ourselves, but by some property of the bodies whereby they pass without other agency from the one state to the other. I will, first of all, consider this passage of the liquid to the gaseous state. Heat is absorbed, and is, therefore, abstracted from any body which is in contact with the body so passing from the liquid to the gaseous state, and the first body is thereby cooled. The examples of this kind of source of cold are exceedingly familiar. You blow on your hand to cool it; if your hand were as dry as parchment, so far from cooling your hand by blowing on it, you would actually heat it. But moisture evaporating carries off heat. You all of you know that the passage of a liquid into the gaseous state is governed by two main things. One of these is the temperature and the other is the pressure. The pressure remaining the same, the liquid passes more freely into the gaseous state when the temperature is increased; and the temperature remaining the same, the liquid passes more freely into the gaseous state when the pressure is diminished. Now, there are various ways of diminishing the pressure which is being exercised upon the surface of a liquid. I will take the familiar example of a little porous cup, a little unglazed earthenware cup, containing water. The water penetrates into the pores of the cup, and presents therefore a great surface for evaporation; but the water is now being restrained by the pressure of the air. If that pressure be removed and kept withdrawn, the evaporation takes place so rapidly that the liquid passes into the gaseous state; to do so it requires heat, and that heat it gets from the water which remains behind, which presently freezes. In order that the evaporation of the water which otherwise would fill the glass receiver may be removed, some liquid very eager for water, such as oil of vitriol, is poured round the evaporating water, so that the vapour of the water as it rises out of the porous cup is at once laid hold of and removed. The diminished pressure is maintained, the evaporation takes place continuously, there is a continuous passage from the liquid to the gaseous state, heat is therefore continuously required for that passage, and it becomes a source of cold and freezes the water.

Here is a similar experiment which is just commencing. But instead of withdrawing the air by means of the air-pump, the vessel before us has been cleared of air by boiling water. The apparatus consists of two glass bulbs joined together by a glass tube. Then the air being all out, the water is shaken to one end; there is nothing here but water and vapour of water. The vapour of water at the ordinary temperature is restraining the water from further evaporation; but if that vapour of water is removed by solidifying it, and this can be done by surrounding it with a source of cold or freezing mixture, then evaporation takes place; in order that the water should pass into the vaporous state heat is required. The heat is obtained from the water itself, and the water is beginning to freeze.

You have a more perfect example of the same in this air-pump of M. Carré. This is precisely the same system as the ordinary air-pump; the water in this flask has to be frozen. The flask is attached by a little india-rubber stopper to the air-pump. By the air-pump the air is withdrawn, and the water begins to boil because the pressure is diminished. But what becomes of the vapour of the water? It is absorbed at once by the oil of vitriol contained in this horizontal black tube; and this pump is so perfect that in about three minutes the water begins to freeze, and in a few more minutes the whole will be converted into ice. There you have the heat, which is necessary for the melting of ice, withdrawn from the water, and the ice is formed. The vapour of the water as it rises is absorbed by the sulphuric acid, so that the vacuum is maintained even better than in the ordinary air-pump.

That is an example of the passage of a liquid into the gaseous state demanding and obtaining heat, or depriving its neighbours of heat and freezing water. You see already the water is beginning to boil, but its temperature is far below what it was before, because the heat is now going out of the water into the steam. The vapour of the water therefore obtains heat in the act of becoming vapour, and that heat it gets from the residual water. In a few minutes therefore a sheet of ice begins to form, and then the heat penetrates through because the evaporation

goes on from the ice; I will not say as well as it did from the water, but still continuously.

Then you will see that this diagram may be a little continued to the right; that is, not only in the passage of the solid to the liquid state, not only in the passage of the liquid to the gaseous state is heat required, but also for the expansion of a gas. You take a certain quantity of air, and you require to put heat into it in order to make it bigger. Supposing you take a quantity of air at a certain temperature but compressed more strongly than air ordinarily is, and you allow that to expand; in expanding it pushes away the air which is pressing on the orifice which you open: it acquires a greater volume in expanding, of course, and in order to get that greater volume it requires heat, and that heat it gets from surrounding bodies. Here is one experiment I will endeavour to show you. I have here some air condensed in this bottle; it has been allowed to get cool, because in the act of condensation heat is set free; then I will allow it to expand and play upon the surface of this thermopile. I need not here explain that instrument, but will only show you the effect which I wish to produce. You will see either directly or by reflection in the mirrors the image of a magnetic needle around which a wire is coiled, the ends of the wires being connected with the thermopile. What I want you to notice is that when I bring my hand, or anything warm, near the face of the thermopile the needle turns in one direction. Never mind the cause of that, which it would take too long to explain, but you see the effect. Now I wish to show you that when this compressed air blows upon the thermopile, the needle turns in the other direction. Whatever therefore my hand gave, the compressed air in expanding gave the opposite. My hand, of course, gave heat, and the air in expanding absorbed heat, and produced cold. That is another method for artificially producing cold. You can pump air into a boiler; in doing so the air becomes hot; let that heat radiate into space, so that you start with a mass of cold compressed air; let that air expand, and since for that expansion heat is required, it is obtained from the surrounding bodies, and you have a source of cold.

Then, again, when a liquid passes into the gaseous state heat is required, and heat must be obtained, and the most familiar example you can possibly have is this ordinary air thermometer. A certain volume of air is enclosed in a glass bulb, with a glass stem which dips into a liquid. Here is some ether which if tested by a thermometer is exactly the same temperature as the air, but which, if freely exposed in a large surface to the air, by its own power of diffusion passes from the liquid into the vaporous state. In so doing, it demands heat, and gets it from its nearest neighbours; it gets it from the glass, the glass cools the air within, which causes a diminution of volume, and allows the atmosphere pressing through the cork of the vessel containing the coloured liquid to overcome the diminished spring of the imprisoned air and to push the liquid up the tube. Examples of this kind are innumerable. I may call your attention now to this flask connected to the air-pump, which is already half frozen, and here also the little porous cup is full not of water, but of ice.

To resume the thread of my argument, there is an example of the application of such a phenomenon as this, the evaporation of the ether as a source of cold, in the Exhibition. A very remarkable development of the method has been made by M. Pictet, of Geneva; it is the same method which has been applied by Mr. Gamgee to the artificial ice rink at Chelsea. Here, instead of using ether, he uses a liquid still more volatile; sulphurous acid, which can only exist at our ordinary temperatures in the liquid state under great pressure. To maintain this as a liquid, it must be restrained either mechanically from evaporating, or it must be kept very cold. About the advantages or disadvantages of this plan, of course it is not my function to speak, but the effect is very striking, and you will see in the Exhibition large slabs of ice formed by the evaporation of this volatile liquid. I need not say that such a liquid as ether or as sulphurous acid which M. Pictet uses, is rather expensive, and of course it would not do to apply them as in my experiment; the vapour must be condensed again into the liquid state, and that can be done. The plan adopted by M. Pictet is to withdraw the atmospheric

pressure from the surface of the liquid, passing it as a cold vapour through a worm surrounded by a liquid which is not easily frozen (dilute glycerine), and then by means of a pump compressing that vapour again until it becomes liquid and restoring it to its original place, so that by a sort of double action, pressure is removed on one side and increased on the other. The vapour of the liquid as it is withdrawn grasps the heat from the surrounding bodies and cools them; as it is compressed again, it has to give out that heat certainly, but that can be allowed to radiate freely, and the quantity of heat absorbed from the surrounding bodies is measured by the working of the air-pump. The water to be frozen is in metal cases in the glycerine.

In the second place, I will return to the older sources of cold, which are called freezing mixtures, caused by the passage from the solid to the liquid state. It is familiar to you all that when you scatter salt on snow in front of your doors, you have a liquefaction, but we all know that that liquefaction is not a fusion in the same sense that lead is melted by putting heat into it. You know that the semi-molten mass of ice and salt is colder than snow itself, and of course colder than salt. You have here an instance of two solids, by some force or other, forming one liquid, brine. Here you have the degeneration of two solids into liquid, and when a solid passes into the liquid condition, and in this case when two solids become one liquid, heat is required; that heat is got from the neighbouring bodies, and you have a freezing mixture.

Now, the proper explanation of freezing mixtures has been to say the least very obscure. I believe some light has been thrown upon it by recent experiments, and I can best introduce the true explanation of the process which goes on in a freezing mixture by starting with this fact, that whenever a salt is soluble in water at o°C., that is, at the freezing point of water, it is also soluble at some degree, or fraction of a degree below o°C. In fact, if you take a solution of any salt whatever and cool it, you can always cool it to o°, and you can always cool it lower than o° without solidifying either all the salt or all the water. That is the fundamental fact upon which some

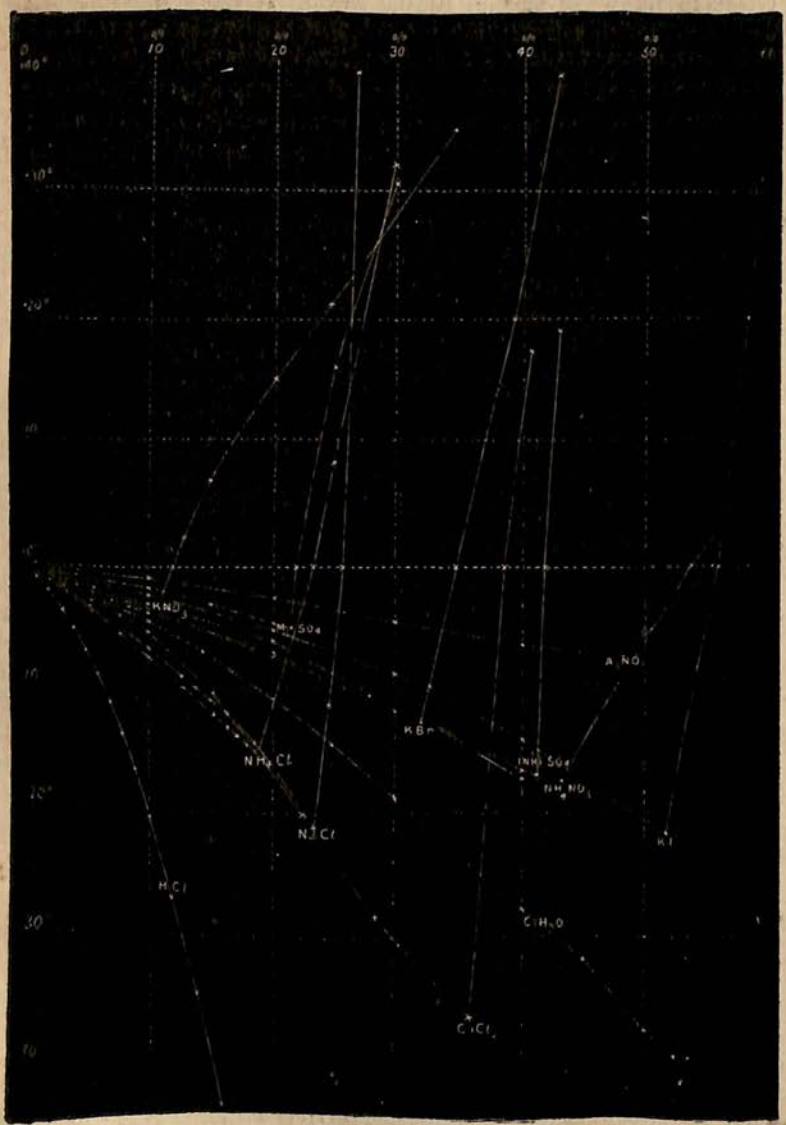

KNO3
(NH4)2SO4
NH4Cl
NaCl
KBr
(NH4)2SO4
NH4NO3
AgNO3
KI
HCl
C6H12O6
C6Cl6

FIG. A.

other facts depend. To understand the rationale of a freezing mixture, I refer to this graphic figure, copies of which I have placed in your hands. (Fig. A.) Some of you may not be sufficiently acquainted with the symbols that are used here, so I will explain them. Supposing you take distilled water; that contains no per cent. of any salt, it freezes at 0°C. The central dotted line running horizontally across the diagram represents 0°; the heights above and the depths below represent degrees of temperature above and below, whilst the degrees to the right represent the percentage of salt in the salt solution. Here is common salt, or chloride of sodium. If you start with distilled water, that solidifies at 0°. If you take a 10 per cent. solution of common salt, that is, 10 ounces of common salt, in 90 ounces of water, you will find that the solidification begins at —7°; the solid so formed is pure ice, and the stronger the brine is with which you work, the lower is the temperature at which the ice begins to separate out, and so on until you come to a certain strength; which is about 23 per cent. of salt. Then no longer does the ice separate out as such, but in combination with all the salt which is there. If you take the same solution, a very weak solution, say 10 per cent., from which ice separates out at —7°, let it separate out and go on cooling it: more and more ice will separate out, the brine will get stronger and stronger, until it reaches this strength of 23 per cent., and then it stops strengthening and solidifies as a whole. Its composition remains constant, its temperature remains constant, it solidifies just as much as a whole as ice itself does. If you take sal-ammoniac, and start with a weak solution of that, and strengthen it by the removal of ice until you reach a certain strength, not so strong as in the case of common salt, only about 20 per cent., then at that temperature, which is not so low as before, about —16° instead of —22°, you get the solidification at a constant temperature, and a body which solidifies in a constant composition. Here again is hydrochloric acid, and these are the percentages. I have not succeeded yet in getting a solid compound of hydrochloric acid with water. So with these other salts. By withdrawing the ice you at last reach a certain strength, where the

ice and the salt solidifies together in a definite ratio and at a definite temperature. These bodies can only exist in a solid form below o°C., and therefore it has been proposed to call them cryohydrates, or frost hydrates. You get the same thing if you start from a strong solution. Here for instance is nitrate of silver. You take a weak solution, by removing the ice; you gradually strengthen that, until you get the cryohydrate of the nitrate of silver which solidifies at about —8°, and has a strength of about 48 per cent.

But now supposing that instead of starting with a weak solution, you start with a solution which is saturated at o°. Supposing you saturate water at o° with lunar caustic or nitrate of silver, and then cool the solution. A saturated solution of nitrate of silver contains 55 per cent. If you cool that, the body which separates out is not ice, but it is the anhydrous nitrate of silver. You, therefore, in cooling the solution starting at o° impoverish the solution by the withdrawal of the salt itself, until you reach inevitably the same ratio, the composition of the chryohydrate, whereupon the solidification ensues at the temperature peculiar to that salt, and of that constant hydration. Table B. shows several salts, beginning with bromide of sodium, a body very much like common salt; here is sal-ammoniac, saltpetre, corrosive sublimate, alum, and others; all have been examined in the same way, and you find that each of these salts contains the percentage shown on the table, when it solidifies with water as a chryohydrate.

The existence of these cryohydrates has thrown considerable light on the action of freezing mixtures. How is it that if you mix saltpeter with snow you get a freezing mixture, but if you mix common salt with snow you have a much more powerful freezing mixture? Whence this difference? It arises in this way. Supposing you take a mixture of saltpeter and snow, under the most favourable circumstances you can only get a depression of the temperature to 3°. How is that? because when you mix the saltpeter with the snow, you of course get the solution of the saltpeter in the water. If that temperature sinks below 3°, then you will get the solid cryohydrate of salt-

TABLE B.—Showing (1) the chemical formula of the salt, (2) the lowest temperature to be got by mixing the salt with ice, (3) temperature of solidification of the cryohydrate, (4) molecular ratio between anhydrous salt and water of its cryohydrate (water-worth or aquavalent), (5) percentage of anhydrous salt in portion of cryohydrate last to solidify.

TABLE B.

(1) Formula of salt.	(2) Temperature of cryogen.	(3) Temperature of solidification of cryohydrate.	(4) Molecular ratio or water-worth or aquavalent.	(5) Percentage of anhydrous salt in last cryohydrate M. L.
	°	°		
NaBr	−28	−24	8·1	41·33
NH_4I	−27	−27·5	6·4	55·49
NaI	−26·5	−30	8·6	49·2
KI	−22	−22	8·5	52·07
NaCl	−22	−22	10·5	23·60
$SrCl_2+6H_2O$	−18	−17	22·9	27·57
$NH_4{}^2SO_4$	−17·5	−17	10·2	41·70
NH_4 Br	−17	−17	11·1	32·12
NH_4NO_3	−17	−17·2	5·72	43·71
$NaNO_3$	−16·5	−17·5	8·13	40·80
NH_4 Cl	−16	−15	12·4	19·27
KBr	−13	−13	13·94	32·15
KCl	−10·5	−11·4	16·61	20·03
K_2CrO_4	−10·2	−12	18·8	36·27
$BaCl_2+2H_2O$	− 7·2	− 8	37·8	23·2
$AgNO_3$	− 6·5	− 6·5	10·09	48·38
Sr_2NO_3	− 6	− 6	33·5	25·99
$MgSO_4+7H_2O$	− 5·3	− 5	23·8	21·86
$ZnSO_4+7H_2O$	− 5	− 7	20·0	30·84
KNO_3	− 3	− 2·6	44·6	11·20
Na_2CO_3	− 2·2	− 2	92·75	5·97
$CuSO_4+5H_2O$	− 2	− 2	43·7	16·89
$FeSO_4+7H_2O$	− 1·7	− 2·2	41·41	16·92
K_2SO_4	− 1·5	− 1·2	114·2	7·80
$K_2Cr_2O_7$	− 1	− 1	292·0	5·30
$BazNO_3$	− 0·9	− 0·8	259·0	5·30
$Na_2SO_4+10H_2O$	− 0·7	− 0·7	165·6	4·55
$KClO_3$	− 0·7	− 0·5	222·0	2·93
$Al_2NH_4\,2SO_4+12H_2O$	− 0·4	− 0·2	261·4	4·7
$HgCl_2$	− 0·2	− 0·2	450·3	3·24

peter separating out. That governs the minimum temperature attainable; and hence it is that if we mix any salt with ice or snow, we find there is partial liquefaction. If we examine the composition of the liquid portion, we find it invariably to have the composition of these cryohydrates. If the temperature sinks below that, the cryohydrate solidifies, the solidification gives out heat, and stops the sinking of the temperature. Therefore no freezing mixture can give a lower temperature than the temperature at which the cryohydrate of the corresponding salt solidifies.

Having spoken of these bodies, there are one or two I should like to show you. First of all, it is clear that by means of a freezing mixture of ice and salt one can never obtain the cryohydrate of salt itself; the temperature obtained by salt and ice in a freezing mixture is just insufficient to solidify the cryohydrate, but the temperature obtainable by common salt,—22°, is abundantly sufficient to solidify the cryohydrate of sal-ammoniac, and accordingly you have here a freezing mixture of common salt and ice, and on putting into it a tube with a solution of sal-ammoniac you see beautiful crystals of this compound of water and that salt. Here is the solution of common salt which is in the right proportion to solidify as a whole, but the temperature is just insufficient. To get solidification, therefore, I must employ some more powerful freezing mixture, and the one I will employ will illustrate very fairly the first diagram. I have here some liquid carbonic acid, which, like the sulphurous acid of M. Pictet's freezing machine must be repressed in order to maintain itself at this temperature in the liquid form, and it is, therefore, contained in a strong wrought-iron bottle. On removing the pressure something will happen, something similar to that which happened under the bell jar of the air-pump; that is, when the pressure is removed the liquid will pass into the gaseous state; and just as that passage of the liquid into the gaseous state solidified the residue of the water and froze it, so here the passage of the liquid into the gaseous state solidifies the residue of the liquid, and one gets solid carbonic acid. In order to avail oneself of its extreme power of withdrawing heat from bodies, one must make it touch those bodies, and that is done by mixing

it with a non-freezable liquid, namely, ether. Here is the solid carbonic acid. I will mix this snow-like carbonic acid with ether and make a sort of paste, which merely serves to bring it into close contact with the tube containing the ice and common salt. You see immediately the temperature lowered below that proper to this freezing mixture, and you get the solid compound of ice and common salt formed. The temperature is—22°. The temperature in fact obtainable by this freezing mixture is that adopted by Fahrenheit as the zero of his thermometer scale, or nearly so, and it is as constant as that of melting ice itself. In this Table B. there is a list of salts which have been tested in this way, and opposite them are the temperatures obtainable when they are mixed with ice or snow, while the second column gives the temperature at which the cryohydrate of the respective salts solidifies. You will see at a glance that these numbers are, within experimental error, identical with those in the first column; proving, I believe, as conclusively as any physical fact can be proved that the temperature of solidification of these cryohydrates is the real circumstance which conditions the temperature of freezing mixtures.

You will see now that in the experiment with the two glass bulbs the cold has been transferred from the freezing mixture of ice and salt to the other bulb, as if the cold had passed from one into the other, and it is therefore called a cryophoros. In reality, of course, the heat has passed from the second to the first; in the opposite direction. It is not this which has gained cold, but that which has lost heat; it has been deprived of heat by the vapour which arose from its surface, and which passing into the vaprous state gained heat and got it from the water, and the vapour passed over and was condensed by the freezing mixture, so that the name of cryophoros is so far a misnomer; it is the heat which passes, and not the cold. I see the time is nearly at an end, and I think it is better to conclude rather before the hour than after. I need scarcely call your attention to the fact that I have merely sketched this latter part of what I have to say, the subject being very extensive, and it is at present by no means sufficiently studied, but if these

few experiments which I have had the honour of bringing before you, at all tend to clear up the hitherto somewhat mysterious phenomena of freezing mixtures, my task has been accomplished.

The CHAIRMAN: Ladies and gentlemen,—I am sure it will be your wish that your thanks should be conveyed to Professor Guthrie for his most interesting lecture. I am quite certain that the experiments which he has made, and the observations with which they were accompanied, will not have frozen any of us, but that I shall be permitted to convey some warmth into the expression of your thanks to Professor Guthrie.

THE METHODS EMPLOYED AND THE RESULTS OBTAINED IN THE LATE TRANSIT OF VENUS EXPEDITION.

By the Rev. S. J. Perry, F.R.S.

June 12th, 1876.

Major Festing, R.E., the Chairman, introduced the lecturer.

The Rev. S. J. Perry: Mr. Chairman, ladies, and gentlemen,—The subject on which I have to address you this evening is one to which great attention has been called during the last few years, and it is one which is excellently represented in this Exhibition. The interest attaching to the subject arises from two things, first from the scientific value of the research, and secondly from its practical nature. I will not enter into particulars at present with regard to the scientific value, or the practical use, but shall say a little of that as we go along; but these two things have led all the civilised nations of the world to equip expeditions in order to carry out these observations, and that alone is enough to show the importance that is attached throughout the whole world to the subject. There is very little time and a great deal of matter, and so you must allow me to go rather briefly through the first steps of the subject.

In the first place I must refer for a few moments to previous transits. The first idea of a transit of Venus came into the mind of Kepler as he was working out those famous laws of planetary motion that he discovered from the observations made with an instrument that we have below. In the lower gallery there is the quadrant of Tycho Brahe, and this Danish astronomer with

that simple instrument, without any telescope, made out the positions of the planets with such accuracy that Kepler from those observations, and particularly from the observations of the planet Mars, was able to discover the laws which regulate the motions of the planets. As he was making these grand discoveries, he saw that two of the planets, namely, those which have their orbits within that of the earth, might occasionally pass across the sun's disc, and might be seen on the sun's disc as black spots. We all know that the planets receive their light from the sun; the brilliant hemisphere of the planet is therefore turned towards the sun. If a planet comes and places itself between the sun and us, then the dark part must be towards us, and the bright part towards the sun, and, therefore, we see the planet as a dark spot on the solar disc. Kepler, in the year 1629, found from a study of the motions of the planets that both Venus and Mercury, which are the two planets within the orbit of the earth, the earth being the third planet in order from the sun, would pass across the sun's disc, and be visible on the disc, one in November, 1631, and the other in December. Mercury was the first, and astronomers were on the look out for this planet, and Gassendi was lucky enough to see it. He was the first astronomer who saw a planet upon the sun's disc, and that planet was Mercury. Mercury passed in the month of November, and, of course, when that discovery was made, when Kepler's prediction had been verified, astronomers were most anxious to see the transit of the other planet, namely, Venus, which was to take place in December. They were all on the look out, but nobody saw it. Venus is much larger than Mercury and much nearer us, and therefore it ought to have been seen much easier, but unfortunately while Mercury passed across the sun's disc during the day time, Venus passed during the night, therefore the sun was below the horizon, at least in our part of Europe; it was visible somewhere in the east of Europe, but in England, France, and Italy, the sun had not risen when Venus passed over its disc, and therefore no astronomer saw that first predicted transit of Venus.

It is easy from a single transit to calculate all future transits,

because we know the time that it takes for a planet to go round the sun; we know the time it takes for Venus to go round the sun, and the time it takes for the earth to go round the sun. I have here a small diagram to assist your memories on which I have placed the number of days that it takes for Venus and for the earth to pass round the sun. In the lower place I have put 365.256 or 365 and a little more than $\frac{1}{4}$ as the earth's time; the tropical year is a little less than $\frac{1}{4}$, but it is not the tropical year we are concerned with, but the sidereal one. It takes 224 days and rather more than $\frac{1}{2}$ for Venus to go round the sun, and $365\frac{1}{4}$ for the earth to go round, therefore Venus is continually catching up the earth, and it catches it up after a revolution and $\frac{5}{8}$. Now, if all the planets moved in the same plane, then every time that Venus caught up the earth she would pass between the earth and the sun, and some of the inhabitants of the earth would be able to see her passing across the sun's disc. But the fact is not so. The planets, although they are all nearly in the plane of the ecliptic, are still a little inclined to that plane, and, therefore, when Venus passes the earth it is sometimes a little above the sun, and sometimes a little below the sun. It is necessary not only that Venus should pass the earth, but that it should be near the ecliptic at the time. If those two things are joined together, then we have a transit of Venus across the sun. It is very easy to calculate when that would take place if we know the two periods. Instead of those heavy numbers we can take other fractions that represent those heavy fractions very nearly; that is, instead of $\frac{224.701}{365.256}$ we can take $\frac{8}{13}$ which represents the fraction very nearly; the next represents it better; and the last $\frac{243}{395}$ represents it very well indeed. What does that mean? $\frac{8}{13}$ means that 8 revolutions of the earth are equivalent to 13 of Venus, and the other fraction means that 243 revolutions of the earth are very nearly indeed equal to 395 revolutions of Venus. The revolutions of the earth are years, and therefore if we add 243 to the period of any transit we shall come to another transit. If we add 8 years we *may* come to another transit; 8 years is possibly close enough, but 243 is always close enough. Now, taking the first predicted transit of Kepler, which was in the year 1631, and adding to that 243, we come to

the year 1874, which was the year in which we lately observed the transit. But not only is 243 close enough, but 8 may be close enough. How do we find out whether it is close enough or not? We have to calculate the change of Venus's distance from the ecliptic for conjunctions 8 years apart, and that calculation was made first by Jeremiah Horrocks, and the credit given to him is due principally to his examination of the tables of the planets, and to his computed result, that in 8 years Venus changes its latitude or its distance from the ecliptic by 22′. Now the sun subtends an angle of a little more than 30′; and since Venus only changes 22′ in 8 years, she might pass over a certain part of the sun at one period, and 8 years afterwards having changed her position by 22′, she might pass over another part of the solar disc. Horrocks made the calculations, and he found the passage in the year 1631 was such that it would allow for a change of 22′ and yet give a transit; and, therefore, he was on the look-out for the transit which was to follow that of 1631, namely, in 1639. He had only completed his calculations about two days before the transit took place. He was then a young man of about 22 or 23. He was able to communicate his knowledge to one of his friends in Liverpool and to another in Manchester. In Liverpool it was cloudy on the day of the transit, but in Manchester and near Preston, where Horrocks was observing, it was fine; and Horrocks and Crabtree, his friend, in Manchester, were the two first to see Venus on the solar disc.

I shall not have time to go into the particulars of that transit, and this is of less moment, as the importance of the transit of Venus was only discovered several years later by the second Astronomer Royal in England, Dr. Halley. He had gone out to the Island of St. Helena to observe the stars of the southern hemisphere, and whilst there he had witnessed the first complete transit of Mercury across the sun's disc. He was the first who saw Mercury enter upon the sun's disc and pass right across it. That was the 4th transit that had been seen, but he was the first who had seen a complete one, and from his observation he got his idea of the immense importance of the transit of Venus. That importance arises from the fact that by means of

the transit of Venus we can determine the exact distance of the sun from the earth. That distance is the great celestial unit; by means of it we make all the great measures in the heavens, the distances of the planets for instance,—they follow at once from one of Kepler's laws and from the knowledge of the distance of the sun from the earth. Then a great many of the laws of nature follow the inverse square of the distance. You have the propagation of light, of heat, of magnetic force, and other forces, such as attraction, all following this law of the inverse square, and therefore, if we know what the distance of the sun is from the planets, we know what action the sun is exercising on different planets, and can form some idea of what may be the state of such planets. Again, from our knowledge of the distance of the sun we are able to measure the distances of the fixed stars, distances which are inconceivable to us unless we enter deeply into the subject. The idea seems to be appalling to us, we can scarcely realise the distance of those stars. If Almighty God at this moment were to destroy all the stars in the firmament, we could go on observing them all for the next $3\frac{1}{2}$ years exactly as we do at present. They are so far off that their light takes more than $3\frac{1}{2}$ years to reach the earth. After $3\frac{1}{2}$ years one or two would disappear, and then a few years later one or two more, but it would take 20 or 30 years before we should lose any appreciable number, and all of us would have been buried long, long before a thousandth part of the stars had disappeared. Again, we get from the knowledge of the distance some idea of the size of those bodies. The sun is not the largest body in the universe, yet few of us have any idea at all of what that size may be. Suppose we had the earth here, and the moon at the distance of 240,000 miles from the earth moving round the sun. The moon and the sun appear to us to be about the same size. But the moon is a mere fraction, about $\frac{1}{49}$ the size of the earth; whereas the size of the sun is such that you could place the earth and the moon at its present distance within the sun, and allow the moon to move round the earth as it does at present, and the whole of that system would lie inside the sun, and you would have 200,000 miles to spare all round. That gives some notion of the size of

the sun. And the knowledge of these distances will, when it tells us rightly the size of the sun, give us some knowledge of those wonderful forces that are continually at work upon the sun, sending out those flames we have heard so much of lately, and those tremendous openings called spots on the sun into which we could throw thousands of earths at once, and not fill up a single spot, and yet those spots are opening and closing sometimes in the space of a few days. We have no notion at all of those enormous forces, but this knowledge of the distance of the sun will enable us to estimate more accurately those forces that are at work in nature.

I must not dwell too long on this subject, but pass on from the question of the importance of the subject under a scientific point of view to its importance in a practical point of view; and on this I will only say one word, as we have a great deal to learn about the late transit. I can convey in one or two words a fair notion of the practical importance of this subject of the transit of Venus, by simply mentioning that by it we correct our lunar tables, and our longitude depends on our lunar tables. If our chronometers are not going correctly, we have only the moon to fall back upon for our longitude at sea: thus, in the present polar expedition, their chronometers may be stopped from the cold, and they will then have to depend on lunar observations. Now, observations only give the longitude by comparing the observations with the tables of the moon, and those tables of the moon include, as a necessary element, the distance of the sun. Therefore, if there is any error in the solar distance, that enters into the lunar tables, and into our longitudes, and thus into our knowledge of positions on the earth's surface. You can see at once what practical use it is to the mariner to know exactly what is his longitude, and therefore to have correct tables of the moon.

Halley not only pointed out the importance of this subject, but he also discovered a method which is perhaps the best method even at present for utilising this transit. I have here a diagram representing the method of Halley; but I will first of all refer you to another, which shows you what takes place in a transit of Venus. It is an exceedingly simple thing. The dark body

represents the planet which comes up to the sun, touches it externally, and then internally, passes across along a certain arc, and then goes off the solar disc. Now we will pass to the next figure, in which we shall see at a glance what is required for Halley's method. I have represented the earth as a rather large circle, but in all these cases you cannot represent the relative sizes of the different bodies. If we represented the earth of any apparent size we could not get the sun into the same diagram, so we are obliged to give up relative sizes. This represents the earth, and the other black circle represents Venus, which ought to be represented about the same size as the earth, and there is the sun. In Halley's method you choose two stations, such that the chords described by the planet across the solar disc shall be as different as possible. There are two things that may cause a difference; first of all a difference in the latitude of the observers, and secondly, the rotation of the earth. I will not enter into the question of the rotation, but we may see very easily how that would affect the question. I will simply point out the difference of position with regard to latitude. There is an observer at "N," which is a northern station, and another at "S," which is a southern station. Now light travels in straight lines, and, therefore, the observer at "N" will see the planet Venus in the direction of this straight line, "N V," and Venus will appear to describe the lower chord across the solar disc; whereas the observer at "S," in the southern hemisphere, will see Venus on the other straight line, "S V," and, therefore, Venus, during the transit, will describe that short arc. By means of the knowledge of those two arcs, we can easily discover what is the distance of the sun. I shall not have time to enter into the details of it, but this method of Halley was pointed out in the year 1677. The transit previous to the time of Halley had taken place in 1639, and if we add 243 years to the first transit of 1631, we come to the year 1874. At those periods the earth and the planet occupied the same relative positions in space with regard to the sun; but as those two bodies go round the sun nearly in circular orbits, if Venus at one position in her orbit is in such a place that the transit is visible, in the opposite position of her orbit she will be in the same relative position with regard to

the earth and the sun, and, therefore, half of 243 will lead us also to a transit, but to a transit on the opposite side. The first transit took place on what is called the ascending node, the point at which Venus comes from below the ecliptic to above the ecliptic. The particular transit of which I am going to speak occurs when the planet is going from above the ecliptic to below the ecliptic. If we add one half of 243 to 1639, we come to the year 1760½; the first transit occurred in December, and if we add half a year, we then get to the month of June, therefore the transits at the descending node occur always in the month of June, and those at the ascending node occur in the month of December.

Now, I will be very brief indeed with the transits of 1761 and 1769. There was one in 1769, for the same reason that we saw there would be one in 1639, because the change of 22′ in the latitude of Venus is less than the sun's apparent diameter. This transit of 1761, which was predicted by Halley, and could easily have been predicted by any astronomer, was observed by a great many persons, who went to different parts of the world. About a year and a half before that transit, the French astronomer Delisle saw that Halley's method would not be very good for that transit. It is not very good unless you can get a considerable difference between the two chords described. If the planet passes across near the centre of the sun, then Halley's method may not fail, but it is not good. Delisle, therefore, studied the matter closely, and discovered a method of his own, and that method I have indicated in this diagram. Here we require two stations, rather east and west than north and south. If you follow along the straight lines as before, when the observer on this side of the earth sees Venus at a distance from the sun, the observer at the other side sees Venus touching the sun. The difference between the two times of the contacts as seen from the two stations will enable us at once to discover the distance of the sun. In Halley's method we require four observations, because we require to know two chords. We know the chords from the observed times. We see how long it takes for the planet to pass across the sun's disc, and since we can represent the sun's diameter in the same unit as the two chords described by Venus at the N. and S. stations,

we can put the whole down on paper, and can find out the distance between the chords from knowing the time. In Halley's method we require the time at N and at S, when the planet enters upon the sun, and when it leaves, therefore we require four observations of contact; in Delisle's method we only require two observations of contact. I shall refer to that again presently, when speaking of the preparations for the transit of 1874; but I must finish those of 1761 and 1769 in a very few words. The first was a failure. Persons went to a great many parts of the world, but they did not spread themselves out enough. Halley's method was not very favourable, and Delisle's failed because they did not know their longitude with sufficient accuracy. By Halley's method you do not require the knowledge of the longitude, but for Delisle's you do; and since the longitudes were not known accurately, the method failed. The consequence was a total failure in the year 1761, but that did not discourage astronomers, and all the nations of Europe sent out still more numerous expeditions for the following transit; and for that transit they spread themselves out well; the observations were very well taken, and excellent results were obtained. It was for that transit that the famous Captain Cook made an expedition, and went round the world; and we have here in this Exhibition the chronometer which was used by Captain Cook in that voyage. It was an instrument which he took on to that island of which I shall have to say a few words later on, the Island of Desolation. This transit succeeded. I will not stay to mention the names of the different persons who took part in it, but I cannot allow one of them to pass without a slight remark. He was a member of the French Academy, and went out to observe the transit of 1761; but he was then very unfortunate, as he was on a French man-of-war, and they wanted to go to a part of India which was in the possession of the English. The French man-of-war did not like to approach the shore when they found the place was in the hands of the English, so they took the unfortunate astronomer back again. Whilst at sea he saw the planet passing across the sun's disc, but he was determined not to be baffled. He would not return to France, but settled down in

India for eight years, and waited for the transit of 1769. It required a great deal of patience, and we should have hoped he would have been rewarded. The way he was rewarded was as follows: he had everything ready on the day of the transit, and on that day he saw nothing. But this was not all. He returned to France. When he started he was a member of the Institute, but when he got back he found they had given him up as lost, and disposed of his place, and, what was still worse, they had also disposed of all his property.

This transit of 1769 was well observed, and the German astronomer Encke determined that value of the sun's distance which we were accustomed to receive in our childhood, namely, 95 millions of miles, or a little more; and it is only a few years ago, in the year 1869, that an English astronomer took up the question again, and the reason why he did so and re-examined the calculations of the German astronomer Encke is shown in this table. It contains a number of values of what is called the solar parallax; that is, simply the angle which determines the distance; if you know the angle you know the distance. The parallax of 8·5 gave 95 millions of miles. Now, Foucault and Fizeau, two celebrated French physicists, whose instruments are in this Exhibition, the instruments by which they made the observations for determining the velocity of light, found that the parallax was 8·8 and not 8·5; in fact, Foucault made it 8·86, and Fizeau made it 8·89, or very nearly 8·9. Hanson, from certain inequalities of the moon's motion, determined the parallax to be 8·9. Mr. Stone, from observations of Mars, found it to be 8·8. Leverrier, who received the gold medal of the Royal Astronomical Society this year for his researches on the planets, from observations on the occultation of Mars by a star, found 8·8. The same astronomer, from observations of the latitude of Venus, found 8·8; the same, again, from 106 years' observations of Venus on the meridian, found 8·8. Mr. Stone, from the displacement of Mars, found 8·9. Sir Thomas Maclear found 8·8; and the German astronomer Winnecke found 8·9. Other observations of a similar nature have been made, and have led to nearly exactly the same results, and we always find 8·8 or nearly 8·9 throughout the whole

list. All these determine the distance of the sun, and all give about the same distance, and they all differ from the distance found from the transit of Venus in 1769, in which Captain Cook took part. These methods are so very different that it is impossible that they should all lead us to the same error; they might each of them separately lead to error, but they would not agree in the same error. In the first we have a rotating mirror without any reference at all to the stars, except to the phenomena of the satellites of Jupiter; then a rotating wheel; Mars, the Moon, and Venus; all leading to exactly the same results. It was evident, therefore, to astronomers that the value got from the previous transit of Venus must be wrong, and, therefore, all confidence in the method was shaken. But Mr. Stone, who was then the first assistant at the Observatory at Greenwich, undertook to recalculate the observations of 1769, and the result of his calculations led him to the value of 8·9, a value which was very nearly the mean of the values found from all the other methods. I have not time to explain fully Mr. Stone's calculations, but he restored the method of the transit of Venus to its former prestige, because he found it gave a value which was at least as accurate as any of the values found by the other methods. After these two transits, the next in order is the transit of 1874, the one we went out lately to observe. And I am going now, with your permission, to follow rather one of the expeditions than all of them together, and I shall follow in particular the one in which I took part myself, which resembles closely all the expeditions that were undertaken by this country. We went to different places, but we were all prepared alike, and our methods of observation were the same. In the balcony outside you will see the instruments that we used, and each of our expeditions were equipped in nearly the same way. All of those instruments have a certain interest in themselves as being connected with the transit of Venus, and from having visited nearly every part of the world during the last two years.

I will commence by throwing on the screen a picture of the Greenwich preparations for this transit. I must first mention that the great question at starting was what method we

should adopt—Halley's method or Delisle's. After I have thrown on the screen the different pictures, I will, if time permits, return to the diagram, and give reasons why we should have adopted one rather than the other method, and refer briefly to the other methods,—the photographic, the spectroscopic, the divided object glass, and the divided eye-piece. We have in this picture the part of the earth which was opposite the sun at the beginning of the transit. It was in December, and then the sun is about $23\frac{1}{2}^{\circ}$ below the equator. At the beginning of the eclipse it was somewhere at the eastern extremity of Australia. For this map I am indebted to the kindness of Dr. Huggins, President of the Royal Astronomical Society, who has lent me a number of beautiful slides to illustrate the phases of the transit of Venus. The map is a copy of one of Mr. Proctor's pictures of the earth. This astronomer has done an immense deal of excellent work for the transit of Venus in drawing maps; and he has thus enabled us to understand very much better the different stations and the different ways of determining the best position for the observer. I may also mention that he has pointed out a third method of observation, called the mid-transit method; which enables us to increase our number of good stations, which is also very important. We cannot go into the details, but I may say that most of the stations that were good for the mid-transit method were also excellent for Halley's method. If you surround the sun by a cone, and that cone is continued and encloses Venus, and comes up to the earth, and if the apex of the cone lies between the sun and Venus, and you make a perpendicular section of the cone at the distance of the earth you would get a circle, and in that circle you would get, as it crosses the earth, all the phenomena with regard to the external contact. Or again, if you take another cone, enclose the sun, and make it also enclose Venus, and the apex now lies between Venus and the earth, a perpendicular section at the earth's distance would be a circle that gives you all that you would require to know about the internal contact. The sections of those two cones are beautifully drawn by Mr. Procter, and are shown on this map. We have there the lines in which those cones pass across the earth.

Those circles are very large indeed compared with the earth, and therefore they give very nearly straight lines. All places on the earth within those lines would see Venus coming on the sun's disc; then it would pass along and come out at the opposite point. For Delisle's method it was necessary to have the time as different as possible, and therefore not to have stations near one another, but differing much in longitude. Captain Tupman was put at the head of an expedition to the Sandwich Islands, which was very nearly the best point, and we were near the opposite extremity of the same diameter. At one point the ingress was accelerated, and at the other retarded, and therefore the greatest possible difference was obtained. The observations at both stations succeeded very fairly.

We have now another map showing the position of the earth at the end of the transit. If you remember the position of the last map, Australia was in the centre, but you have a part of this map common to the other map, and every part that is common to the two maps had the whole of the transit of Venus in view; but this part, including a great portion of Africa, did not appear in the last map, therefore observers there could not see the beginning of the transit at all; the sun had not risen; whereas the Sandwich Islands are now left on the other side, so that the inhabitants there saw the beginning of the transit, and did not see the end. Therefore, you have a part of the globe in which the beginning is seen, a part in which the end is seen, and a part in which the whole is seen. For Halley's method you must see the whole, and therefore you must choose on this map and the other map those points which are common to the two, and yet these stations must differ as much as possible in latitude, taking into account the rotation of the earth. You can observe that Kerguelen's Island, or the Island of Desolation, appears on this map as well as on the other, and therefore it was a good station for Halley's method. The corresponding N. stations are those of Siberia. We have now on the screen some of the houses that were built at Greenwich, in preparation for the transit, for it was not only necessary to study the best positions on the earth's surface, but also to invent instruments, and to make them, and

to build houses. Some of these were carried nearly to the frozen region of the South, others to New Zealand, others to the middle of the Pacific Ocean, to Egypt, and so on. It was necessary to have these all ready, and then to train the observers, so that there was an immense amount of work to be done in preparation. The burden of that fell, of course, upon the Government representative of Astronomy in this country, Sir George Airy. The whole of the southern part of the grounds at Greenwich was filled with these huts, and so was the wilderness in Greenwich Park, and there was again a large village of wooden houses in the Vicarage garden. All the instruments had to be tested to see that they were correct, not only for any one place, but for the particular place they were destined to occupy. There is now on the screen a picture copied from a paper copy of a Janssen plate. All round the margin are successive pictures of the same sun spots taken at intervals of one second. This is Dr. Janssen's method. You see at one portion a little square opening. The plate is brought successively opposite the same point, the wheel bearing a sensitive plate, and it is turned round so that every part is brought in succession to this opening, and you get in the course of a minute 50 or 60 photographs of the same thing. It was Mr. Christie who devised this plate for Dr. Janssen's method, and it worked admirably; we were thus able at the important times of contact to take photographs of the sun with Venus upon it every second. Here we have a similar arrangement in this succession of photographs of the model of the transit of Venus. There was a little triangular opening and the sun was thrown upon that, so that we had a strong sunlight on that little angle, and then an imitation of the planet was made to cross the triangle, and we saw all the phenomena of contact, and studied them at our ease, with this model, before going out. When we had all our things ready, we left early, so as to make preparations on the spot. The transit was to take place in the month of December, and we started in May and June. We made first for the Cape of Good Hope. There was no great difficulty about that. We went down with the mail steamer and met with the ordinary incidents of the voyage.

When we got to the Cape two men of war were appointed to take us to the Island of Desolation. As far as the Cape we were, I cannot say on known ground, but at any rate; in known seas, but when we passed the Cape we got into unknown regions, or rather into regions known to a certain extent, that is to say, known as windy places. We went out in search of a wind, and we pretty soon got one. It carried us along almost as straight as an arrow to our destination, but unfortunately instead of keeping up a good pace, it passed the limits of a good pace, and then we got into a storm as heavy nearly as you could have at sea. Of course it was impossible, surrounded by mist and cloud, to approach an unknown shore in the midst of a dense fog and a heavy storm, so that we had simply to wait. We waited for two days, and during those two days the sea was washing over the vessel continuously. The worst of it was that we had our deck covered with live stock; a large number of sheep and oxen; and they were washed from side to side and dashed against the deck and sides of the vessel, so that one morning we found about 40 dead bodies about the deck. Fortunately we did not lose a single human life, but we had certain little interesting accidents occasionally.

We were dining, for instance, one day and heard a tremendous noise overhead and presently down came a great part of a wave into the middle of the dinner-table, which washed the dinner table, officers, and all away, some on the top, and some underneath, and we saw nothing more of dinner or dinner-table for the next two days. On another occasion a number of officers were under the poop smoking, when the sea came and washed them off, and many of them were completely under water. One of our largest boats was attached to the vessel more than 20 feet above the level of the sea, and it was strongly fastened with chains and ropes. The captain thought it was not secure, and sent a number of sailors into it to lash it with extra ropes; the men had scarcely got out of the boat when an enormous wave came and tore it completely off, and we saw nothing more of it. When the sea abated a little we were able to use our charts, and very easily got into Royal Sound, of which this is a picture. There we

found we were not alone, for there were two small schooners belonging to sealers, who acted very kindly to us and conducted us to the station. This is the main station occupied by the English in Kerguelen.. You see there a rock, which protects the houses. This station was well protected, of course I mean well protected for those seas, for sometimes you could not land, but still the ship was very safe. We had on an average five storms per week, but we managed to do what we could on the other two days, and in this little cove we managed to land all our instruments, without much trouble. Sailors are able to do nearly anything; they dragged the houses and instruments up this incline and handed them over to us, and we erected them on this spot. You see there are protecting hills, and we chose the station in such a way that we had protection from the north and west, so that when the wind was blowing very fiercely upon the ships we sometimes scarcely felt it. The next picture on the screen is to give you some idea of the care we took to determine our stations. It may be necessary to revisit these stations. I do not know whether any persons will feel inclined to visit the Island of Desolation, but we took photographs in different directions with a small pocket instrument I had with me, in order that we might be able to identify again the precise spots if necessary. All these pictures were taken with that little instrument; they were taken on dry plates that I purchased in London. I used them eight or nine months later, and some were developed months after they were taken, so that that speaks very well for these little cameras which you can carry about with you. You can take a picture in two or three minutes, hand them to any one else to develope, or do it at your leisure at any time you like, and thus go over your travels again. This picture represents our different stations and observatories, and there is one in which we have the members of the corps of Royal Engineers who took part with us in the observations. I must say of these men that immense credit is due to them, not only for the work they went out to do, but also for the voluntary work they did. They volunteered to take other observations as well as the astronomical ones. I felt always that we might lose everything on the day of the

transit, a cloud might come and deprive us of all our astronomical observations, but we could secure other things, magnetic and meteorological observations, and the members of the corps of Royal Engineers volunteered to take these observations, day and night, every two hours, whilst they remained on the island, which was five months, and I think we ought to be very grateful to them for what they did. I was enabled by means of their work to bring back a sufficient number of meteorological observations to enable us to discuss various important matters connected with the island.

We will go rapidly through the other instruments. This is a picture of the transit instrument for observing the time; the standard clock may be seen in the corner of the hut. A good observer can easily determine the time to within one-tenth of a second. We carried on these observations every fine night, and you can imagine what our difficulties were, when I remind you that we arrived there at the end of spring, but yet we could see nothing of the whole island, as it was all covered with snow. A few days after we arrived we had a fall which left the snow about one foot thick on the deck. They do not leave any incumbrances very long on the deck of a man-of-war, so the snow must have come down very rapidly. Another thing which will show you that the snow fell very rapidly was this. One morning it was very calm, which did not often happen, and the snow had been falling in very large flakes, and those who got up early in the morning between six and seven saw the sea all covered with snow; there was one beautiful plain surface of snow in every direction; then a little breeze sprang up, and in an instant the whole of the snow had disappeared.

The next picture is that of the most important instrument we took out. Some persons spent night after night with this instrument. The other instrument offered some difficulties, as you had to sit and wait for stars for three or four hours sometimes, with the snow falling upon you, before you got a single star in the middle of the field, and that in the middle of the night was not very pleasant work. With the last instrument we got 19 moons out of a total we had wished to get of 30; but with this instrument we were to have got 200 moons or 100 double observations

of the moon, because with this telescope you could see the moon in any part of the heavens, whilst with the other you could only observe it on the meridian. I have actually waited for hours for the moon, it has come into the telescope, and then disappeared before it came to the wire. With this instrument you could follow the moon and take it up at any place. It was so excellent an instrument that you might depend on the latitude determinations to within a few yards. It is the special altazimuth devised by the Astronomer Royal for the Island of Kerguelen. This is a large telescope for observing the transit; it has an aperture of over six inches, and was the one I used for observing the transit and for taking my micrometer measurements during the time. This is one of greater interest, the photoheliograph. I have represented the hut in which the instrument is placed, the sun's rays fall on the object-glass and are bought to a focus at the eye end of the telescope, and are there photographed. Nearly all this work was intrusted to the Royal Engineers. Unfortunately, the clouds interfered very considerably with photography at our station, but others were more fortunate. Here again is an instrument that you will not find outside, because it was not one of the English instruments, but is of great interest when compared with the last. It is the American photoheliograph of which we have heard a great deal. It consists of a telescope with no tube. Here is the object-glass; and here you have a heliostat, an instrument like those downstairs. This consists of a plain mirror which is turned by clockwork, and always sends the rays of the sun in the same direction wherever the sun may be. It sends the rays through the object-glass which is fixed, and then they travel along to where the eye-piece of the telescope would be, but there you have a photographic camera in a dark room where all these photos of the sun were taken. There is a great discussion as to the respective values of this instrument and the last; but I must not stay to discuss it.

When we had erected all the instruments at our first station, which occupied a very considerable time, we began to think of other stations, and the first we settled upon was at a distance of about six or seven miles, and the person in charge of it is represented in this picture. He was one of the chief officers in the Expedition;

unfortunately he caught the fever the other day a St. Helena and died, greatly to the loss of the navy and of science. He took some of the best observations made in the Kerguelen, and I have no doubt they were as good as any in the whole of the Expedition. The observatory was devised by himself at the Cape of Good Hope. I will show you now a few pictures of the observatories at the three stations. There was a great deal of discussion about MacDonnell or Heard Island, an island discovered about the the same time both by MacDonnell and Heard. This station was to have been occupied by both the Americans and the Germans; the English chose Desolation, and the Americans intended to occupy the Crozets, which were not far off. These were the only points excellently suited for the method of Halley. Heard Island was given up by the Americans and by the Germans on account of the bad reports received. The Crozets were to be occupied by the Americans, but when they arrived the American captain finding the weather too bad, said he had not time to wait, but took them on and landed them in Tasmania instead, which was a different thing, as this last was no a first-class station. He had not time to wait because the Americans had sent out all their five expeditions in one vessel, whereas the English sent out one expedition in two vessels. Thus the Crozets were not occupied and Heard Island was not occupied, and all the observers were collected together in the Island of Desolation, and at the same end of the Island. The consequence of this might be that a single cloud would prevent us all from seeing the sun on the day of the transit, and we should have spent thousands of pounds for nothing. So, I thought it was time to make an effort. We had two vessels, and I asked the captain if he was prepared to occupy Heard Island as well as Desolation. He expressed his willingness, and not only that, but he got his vessel ready. I had asked the opinions of all the different persons we had met with, and they were all against it, but not sufficiently against it to make us give it up. There was one other person we had to see who was the greatest authority on the point, Captain Fuller, and when we saw him, unfortunately he told us we could not land except on vary rare days, one or two in the

month, and then we could not land in our own boats, but should have to use the boats of the whalers, and our instruments, to say nothing of the observers, would have to be practically under water, whilst we were going on shore. I thought chronometers which had been under water would scarcely give us the time within $\frac{1}{10}$ of a second, so that finally we were obliged to give it up. It was a good thing we did so, as I was thus able to choose another station where we saw the whole of the transit, whereas in Heard Island nothing at all was seen. This is the cheerful side, the summer picture of the third station. To find the latitude and local time I had to stay there for a short time; an officer volunteered to go with me, and two non-commissioned officers completed our staff. As soon as the sun set I had to leave this tent to go on to this heap of stones, and remain there until the sun rose, and then get into that little tent and do the best I could until breakfast time. Here is another picture of the same station with the entrance to Royal Sound. This one represents the German station. We have already seen the American instruments and station. This is another picture of the German station. We were somewhat astonished at the Germans placing themselves in what looked like a graveyard, but we thought that what appeared like tombstones must be meteorological stands; but we finally found it was really a graveyard. Kerguelen was once a large sealing station, and they had had to bury a great many of their men at this place. There were five stations occupied altogether by different nations, and I may say at once that we were fairly successful at all five.

I will now show you some of the slides also kindly lent me by Dr. Huggins, showing the different phenomena of the transit. This represents the sun. You see the planet entering upon it, but you do not see the planet until it is on the sun's disc: A great many talk about seeing the first contact, but you cannot see it, there is nothing to see; and though some people telegraphed home to say they had seen it, the contact was over some time when they obtained their first glimpse of the planet's outline. The important times are, when it appeared first, just when it touches the limb, and when it passes off. This

represents the relative size of the sun and planet, and the arc which would have been described from the centre of the earth. There was a phenomenon spoken of a great deal in the former transits which seemed to get in the way of observers, called "the black drop." When the planet came on to the sun's disc, it got deformed into a pear shape, and was drawn out towards the limb of the sun. There was a great dispute whether that was due to the instruments, to the atmosphere, or to the observer; or whether it was something in the phenomenon itself. It has been proved, I think, very satisfactorily, that it was due either to the atmosphere, or to the instruments, or to the observer, because out of an immense number of skilled observers who went out this time, very few indeed saw anything of the "black drop." Some may have seen a slight shadow between the planet and the sun's limb, but very few indeed saw the planet drawn out; there were a few; but that might have been due to the atmosphere, or to their instruments, or to something else; in the Himalayas there were observers at different heights, those at the greatest height did not see the "black drop," and those below did see it. In Calcutta there were two observers side by side, one saw something of the black drop, and the other did not see it at all.

I said just now that the first external contact could not be seen until the planet was already partly on the solar disc, and therefore it had not been seen at all, but there is a method of seeing it, and one of the French astronomers who went out did see it very well. He saw it by enabling himself to see not only the sun, which is represented by this pale yellow, but also the chromosphere, that red envelope of the sun that takes all those fantastic shapes. If he could see the planet creeping over that, of course he could see it come up to the sun's disc, and see it touch, and that would be the external contact. Dr. Janssen, by his knowledge of the bodies contained in the chromosphere, compared with the bodies contained in the sun, was enabled to do this. The sun itself gives a complete spectrum, almost continuous from red to violet, but the chromosphere gives only bright lines separated by long intervals of darkness, and thus by using a coloured glass which would cut off a great deal of

the light of the photosphere, and very little comparatively of the chromosphere, he was able to see the planet coming along the chromosphere. Of course the observations at egress are the same as at ingress, only in inverted order.

These are two pictures which Sir George Airy has lent me, which were taken during these observations. This represents the sun and the planet. These pictures have been measured very accurately, and others are being measured at present in different places.

Before summing up the results obtained by the observations, I will now give you, if I can, an idea of the Island of Desolation.

From these pictures of the island you see that even when there was no snow you could not go one pace without fixing your eyes on the ground, and being careful not to tumble over the blocks of stones. Here is another picture showing the rocks overhanging the sea, in which the birds build their nests. It was sometimes rather dangerous taking these nests. One sailor fell 30 feet and knocked a piece of bone an inch long into his skull, then he rebounded about 30 feet lower down, which shook the piece out again, and that second fall saved his life. To show you what British tars are made of, I might mention that when he got back to the vessel he would not let anybody help him over the side, but climbed up himself and gave his salute to the officers before being carried down below.

It was a difficult thing to walk even a few miles, on the boggy or rocky ground of the island, and two officers at the second station nearly lost their lives in going from one station to another. They had broken a spider line, and as there were no spider lines with them, they had to bring part of the instrument to the first station for me to put a new spider line into it. To do this, they had to walk six miles. They walked two or three miles, and then came to an arm of the sea; these arms run in for a distance of about 12 miles. They walked round one arm of the sea and then their provisions failed them, for they had not taken much for what they thought a walk of about six miles. Then they came to a second arm of the sea; it was so dark they were afraid to stir,

and they had to sit down there and wait until morning, when they determined to swim across. Fortunately where they jumped in they found it was only up to their middle, and so they were able to wade to an island which was near. Then they had to get into the sea again, when finding that was out of their depth, they swam in nearly frozen water for almost a quarter of a mile. At last they got into seaweed, which sometimes grows to a length of 40 feet, and there swimming was impossible, but they were strong young men and they pulled themselves through by means of the seaweed, but when they got to the house they were very nearly exhausted. Some 8 or 10 hours' rest put them all to rights again. They were glad enough to go back again in the boat, but it was rather dangerous sometimes going by sea in small boats. On one occasion on a perfectly calm day, a gust of wind suddenly struck a boat and capsized it, leaving the men no option but to clamber on to the keel and sit there. There were two islands between them and the open sea, the first of which was rather the largest. They hoped to reach the first one, but when they came near to it the current carried them away; they were perfectly helpless, simply sitting on the keel of a large boat. Happily they managed to touch at the second island, and there they had to land as best they could. The ship missed them in the evening, and next morning sent out a party in search of them. Fortunately they were soon found, but they had already spent a night in the open air, their clothes all soaked through and through, and the temperature at freezing point. Here is another picture giving you some idea of the rocks, and here is one of the birds we found there. On the island there was not a single tree or shrub, but a great quantity of cabbages, a bitter sort of cabbage which was very good after being boiled several times in different waters. It was an exceedingly healthy place, and we attributed this mainly to the cabbages; I do not think that on the average more than two men at a time were on the sick list in a large man-of-war. But although we had no trees and no bushes, we had an immense number of birds of every size. This is a dark sooty albatross. Two of these birds built their nest very favourably for some goats which we took out by the advice

of Captain Nares, and some of our sailors found two young kids comfortably reclining on the soft bed that had unwittingly been provided by the smothered hosts. These pictures give you some notion of the places we visited. Of course the places visited by other expeditions were not the same as ours, each had their own points of interest. I will now point out very rapidly on this map the different places which were occupied by the various observers. (The Lecturer then pointed out on the globe and also on the map the stations occupied by the English, Germans, Americans, and Russians.) With regard to the successes of these different expeditions I will merely say a very few words. Of the 32 Russian stations 19 failed entirely, 8 succeeded partially, and 5 were perfectly successful. Of the English stations those in Egypt and Rodriguez were as successful as they could be. The Sandwich Islands and Kerguelen were also fairly successful. At first external contact, we got a certain number of observations, but these we need not count, as we were unprovided with the tinted glass used by Dr. Janssen, and we did not employ our spectroscope. We got three observations of the second contact, three of the third contact, and three of the fourth contact, so that we are able to apply the method of Halley and also that of De Lisle.

It is not true, as has been said in the papers, that there was not a single station at which we got both contacts. At each of our three stations we obtained some contacts. At the first station we got the third and fourth contacts by two observers; at the second station, the first and second contacts by two observers; and at the third we got all the contacts by one observer. Besides, it is well to remark, that the three stations were sufficiently near together for the results to be combined for Halley's method. Then again we were able to take a certain limited number of photographs, but it was an unfortunate day for us in some respects, because we had had such fine weather before. We were looking out for the planet upon the sun, as the time was drawing near. We had a nearly perfect sky, no cloud, and then at last a small one appeared, and the wind was blowing softly, and this cloud came up exactly in the direction

of the sun. When it arrived at the sun, we had still 10 minutes to spare; and it was travelling at such a rate that it seemed certain to pass off the solar disc before the 10 minutes had expired, so we felt a certain amount of consolation. But now this wind, which generally blew almost a hurricane five days in the week, during the whole five months of our stay, ceased altogether, and the cloud remained over the sun for 20 minutes without stirring at all. The consequence was, that we missed our first and second contacts. At the other stations they were more successful, and got their three observations which I mentioned just now. At the Sandwich Islands they could see only the beginning, as they were not situated on the illuminated hemisphere at the end of the transit. In Egypt they could only see the end, but they obtained both contacts at egress. In New Zealand, which was the place of all places where the weather seemed beforehand certain to be fine at that time of year, they saw next to nothing. It was the one place which was quite cloudy during the whole time. They were able to get a few photographs and a few measurements, but very few indeed. Kerguelen was supposed never to be seen except in a mist, but we got very fair weather there. The French astronomer at St. Paul's did not see the sun once during the whole time he was there until the day of the transit, and then there was not a single cloud, and he saw everything perfectly, whereas a friend of his who had gone down to Campbell Island not only saw nothing, but was very lucky indeed to escape with his life. The observations in Egypt were a success; in Japan and in China they were very fairly successful. They are measuring the photographs now, and calculating the longitudes, and the times of contact and so on, and those calculations must go on for the next three or four years. At the last transit they took 100 years before they gave us the results we have now, and therefore we must be content to wait for two or three years for the perfect result of this transit. The French values are coming out very accurately, and the measurements at Greenwich are much better than was expected. Each nation will first work up its own results, and then the results obtained by the different nations will be combined, and we shall get a final value for the parallax

of Venus and the distance of the sun. Sir George Airy has worked out an excellent method for combining all these different observations into one set of equations, so as to give the best result from all the possible observations of every sort that have been taken. For the one point of the solar distance we must wait; the observations have been taken, and we shall get the results later on. But we are already in possession of a great deal. First of all we have the discovery of Dr. Janssen, that by placing a certain tinted glass in front of the sun, we can cut off sufficient of the rays, without interfering too much with the chromosphere, to see the planet as it creeps across the chromosphere, and therefore we have saved one of the four contacts. Again, the same astronomer compared during the transit photographic contact with eye contact, and he found that there is a considerable difference between the two, and therefore we must take that into account in comparing photographs with eye observations. The Italian expedition is the only one from which we have got complete results so far. Tacchini went out to India solely to study the observations in view of the next transit in 1882. Transits always go in pairs. We were ready to take a great many excellent observations of this last one, but there are other observations, new ones, which we were not sufficiently prepared for. We knew what to expect from contacts observed with the eye and what from photos, but we did not know much about the spectroscope, and I have here some diagrams which will give us an idea of what can be done with the spectroscope. The Italian party did not intend to determine the parallax; but simply went to study this instrument. We have here represented four different methods which can be used in the transit of 1882. The first is that of the radial slit. Here is a representation of part of the sun and the chromosphere around it, with a line representing the slit of the spectroscope. A number of prisms disperse the light, and give the solar spectrum. You have a number of pictures of the solar spectrum in these rooms, and the coloured photograph of the spectrum by M. Becquerel is one of the most marvellous things in the exhibition. In that little slit you have a part of the planet, a part of the

photosphere, and part of the chromosphere. When the light is split up, you only see just the brightest line of the chromosphere which is in that part that you are viewing, and the red part of the solar spectrum. As Venus moves across the sun at egress, that dark portion which is in the slit will be spread out all along the spectrum, and you will see in this red part a dark band, and that dark band will steal across the sun, until at last it completely extinguishes the sunlight, and then you have only this little tongue of the chromosphere. By means then of the normal slit you have a line to observe for the contact, and that line is equally good for ingress, for the first contact, and for the last contact. At ingress the planet first extinguishes the upper part of the chromosphere, and at last comes down to the photosphere. But if instead of placing the slit normally you place it tangentially, you may take in merely the chromosphere; and if you disperse the light very much indeed, you can exclude the photosphere altogether from the field of view, and have merely a line of light, which it is easy to keep exactly tangent to the photosphere, and then you will see the planet creep along that line and can determine the contact very accurately indeed. That was the method adopted by Tacchini, who headed this expedition last year. There is another method roughly represented here; the same slit taking in not merely the chromosphere but part of the sun; you see the curvature of the sun; that is meant for a small dispersion and a small telescope, whilst the other is for a large telescope. Here you have the chromosphere and the sun entering together, and therefore there is a certain amount of stray light which interferes very much with the observation; but I think with care you can manage to get a very good observation with it. Tacchini has obtained by this system results differing very considerably from the eye observations, and that has confirmed the very important discovery, that the diameter of the sun as seen through the spectroscope is not the same as seen through an ordinary telescope. The explanation seems to me very simple, viz. that the foundation of the chromosphere is sufficiently vivid to act upon our retina, and therefore we see ordinarily not merely the photosphere, but also a certain portion of the

chromosphere, and the whole visible disc of the sun is made up of the photosphere and a small portion, the brightest portion, of the chromosphere; whereas the spectroscope cuts off the chromosphere entirely, and gives you merely the photosphere, which would therefore be a little smaller. They found that the contact was earlier by 2 minutes $11\frac{1}{2}$ seconds in both these observations than by the eye observations, that their sun was smaller as it were, and therefore the planet came to the edge of their sun before it came to the edge of the sun as seen through the telescope at egress.

There is another method of using the spectroscope represented here, viz., that of Padre Secchi.' When I was passing through Rome, on my return, I went to see the observations as taken by Respighi, and also by Secchi, and I was very pleased indeed with the results of both. Secchi's method is to place a large prism in front of his object-glass. Now a large prism in front of an object-glass is not a thing every person can purchase for experiment, seeing it would cost a very fair sum of money, say £100 or £200. But if, instead of that, you take a perfect direct vision spectroscope, and place that in front of the slit, you can get the same result. I tried mine. I do not know whether it is a perfect one, but it gave me perfect results. I got a red image of the sun with the rays of the spectrum crossing that image, and I got the bright lines of the chromosphere outside at the same time. By these means you can watch the planet as it crosses the chromosphere and get the external contact very accurately indeed. In the course of my explanations I have passed over some names I ought to have mentioned, but I have done so simply for want of time. I trust this will be considered sufficient excuse, as I have already detained you beyond the usual hour.

TELEGRAPHY.

By Mr. W. H. Preece, M.I.C.E., etc.

June 17th, 1876.

The Chair was taken by the Right. Hon. Lyon Playfair, C.B., F.R.S., M.P.

The Chairman: Ladies and gentlemen,—I have the pleasure of introducing to you Mr. Preece, who, as one of the Electricians of the General Post Office which has now charge of the telegraphs of the country, is admirably fitted to explain the electric apparatus now before us.

Mr. Preece: Ladies and gentlemen,—This collection contains the grandest display of telegraphic apparatus that has ever been brought together. It is quite impossible in this room to demonstrate that apparatus to you, but I hope by indoctrinating you to a small extent into the principles of telegraphy to enable you to appreciate the character of the small apparatus that is found in the cases downstairs.

What is telegraphy? It is the art of conveying to distant places by the aid of the ear or the eye the first elements of written language. It does not convey from place to place ideas, but the letters of the alphabet and numerals, so that the different systems of telegraphy simply consist in the different ways in which the letters of the alphabet are formed. When that is done by electricity, we have the electric telegraph. The alphabet must be formed in such a way as to appeal to the mind through the senses, and the two senses that are used are those of sight and of hearing. Owing to the peculiar construction of this room, and the difficulty of rendering visible to you the small apparatus that is used for telegraphy, I have selected to-night the acoustic system; that is, that system which appeals to the ear.

Now, how can we form an alphabet by means of sound? If you listen to a piece of music you will find that there we have sound divided into periods varying in duration, separated from each other by spaces of silence, and also varying in tone, so that every little boy who whistles 'Tommy make room for your Uncle,' performs precisely the same operation as we do in telegraphy, that is, he divides sound in such a way as to produce varying degrees of tone and varying periods of time. Now, we cannot in telegraphy make use of all those variations of tone; we are reduced to simply two notes—one called a dot, which is equivalent to a quaver in music; and the other called a dash, which is equivalent to a crotchet. In order to illustrate this to you I have taken the homeliest instrument that I can find, which is nothing more than a penny whistle. I am not going to frighten you with "the vile squeakings of the wry-necked fife," as Shakspeare calls the pipe, but I will simply show you how, with a simple pipe, we can produce two sounds that can be formed into an alphabet. The first is what we call a dot—a short quick sound; the second is a dash—a more prolonged sound. (The Lecturer then illustrated the mode in which the different letters of the alphabet are produced by means of long and short sounds.) Of course we do not in telegraphy use such an exquisite apparatus as the penny pipe, we use something that is perhaps more scientific, but certainly not more convincing to the mind that we can make alphabets out of sound. Instead of taking a pipe, here is a little apparatus which is now sold in the streets of London for sixpence, called a pocket snapper, and it will, I hope, make sufficient sound to be evident throughout the room.

Now I will call your attention to three facts; the first is that we have a dot; the second is that we have a dash; and the third and most important is that these dots and dashes are separated from each other by spaces of time. There are three spaces: the space separating the elements of a letter, the space separating the letters themselves, or the elements of the word, and the space separating the words, or the elements of a sentence. And the telegraphic alphabet based on this principle consists essentially of dots and dashes and these three spaces. On this diagram is a representation of the same facts, appealing to the eye, that is, the

alphabet as constructed of these dots and dashes. A . — ; b — . . . ; c — . — . ; d — . . ; e . , and so on. All the letters of the alphabet are thus formed with dots and dashes, and they are so apportioned as to give the smallest number of signals to those letters which are most frequently used in the English language. For this purpose we went to the printer's fount, and there we found that the letter most frequently used was the letter *e*, and consequently to this letter was given the one dot. The letter *i* was given two dots. To *t* one dash, to *m* two dashes, and so on throughout the alphabet you will find that the letters that are most frequently used have the smallest number of signals, whilst those which are less frequently used have the greatest number ; but even in the case of *z*, *u*, and *y*, and those letters that are seldom used, in no case is it ever necessary to exceed four signals.

Such being the way in which an alphabet is made, how is it possible that we can form the letters of an alphabet at distant places? If John Smith is in York, how can he record the signs of the alphabet to William Henry in Southampton? To do this we must have some means of producing a force that shall effect our object ; we must have a means for rendering this force evident to the senses, and we must have a means for conveying this force from the one point to the other. I am going to dwell but very briefly on the first point, that is, the means for the production of this force, because I take it that in these days of enlightened education everybody knows something of a common galvanic battery ; such a battery, as you all know, consists simply of a piece of copper and a piece of zinc placed in a cell, which are there exposed either to the action of sulphuric or some other acid. These cells are formed generally for telegraphic purposes in troughs such as I have here. This is an ordinary telegraphic battery consisting of twelve of these cells, and by combining and uniting these in different numbers, we are able to produce currents of electricity of sufficient strength to enable us to transmit signals from one end of England to the other. Batteries are to be found in this exhibition of numerous character. Some for producing motion, some heat, some chemical action. Every work of every sort or kind that is done by electricity requires its own particular battery, and

while one sort is used by Elkington for producing those beautiful silver figures that we see, another sort altogether is required for telegraphic purposes. We have plenty of these batteries under the table, and most of them are of the character of this trough.

Currents of electricity can be made evident to our senses, either by the production of heat, by the production of light, by the production of chemical action, or of magnetism. To-night I intend solely to bring to your notice the effect of electricity which produces magnetism. The electro-magnet simply consists of a bar of iron—in this case two bars of iron—surrounded by wire—and whenever a current of electricity is passed through that wire it converts the iron into a magnet. I have here some small iron nails, and when I put this iron amongst these nails, there is no effect produced, but when I pass a current of electricity round it, you will see it at once catches up some of these nails, and when I cease passing the current the nails fall.

We have, as you see, here the production of magnetism. I want to make a noise with that production of magnetism. Supposing I take a bell; in order to strike that bell with my hand I have simply to hit it with the hammer. Can I impart that same motion to this little hammer by means of electricity? I have a little board, and upon it a piece of soft iron is hinged at its two bottom ends, so that when the attraction of the magnet is brought to bear upon it, the bar will make a motion similar to that made by my wrist. I take the magnet and lay it flat near the piece of soft iron, and on passing a current around it, the attraction due to magnetism produces a motion in the iron. On putting the bell near it and affixing a hammer to the piece of iron, I am enabled to produce sounds. Instead of using the bell to convey to you the alphabet, I will take another means. Here, I have precisely the same board with an armature fixed in exactly the same way, except that it is hinged at its upper extremities instead of its lower ones. I put the magnet in the same position, and I can produce sounds not unlike those produced by this little snapper, and by these means I can produce the alphabet. So you see by the simple production of magnetism in an electro-magnet of this character, we can produce sounds which, by what is called the Morse system, convey the letters of the alphabet. Of

course, an apparatus so large as I have shown you, constructed to make the facts I have endeavoured to explain evident to you all, is not such an apparatus as would be used in a telegraphic office. We have there a smaller and simpler apparatus, and here (Fig. 1) is one of the identical instruments called a sounder, and is so used.

And you will see that by means of this little apparatus, I am able to produce those sounds that make up the alphabet. Of course, merely to produce contact between two pieces of wire, in order to allow the current to follow, is a very clumsy contrivance, only fitted for the lecture table; in offices more finished things are used called *keys*, which are of various kinds and forms suited principally to the taste of the telegraphist.

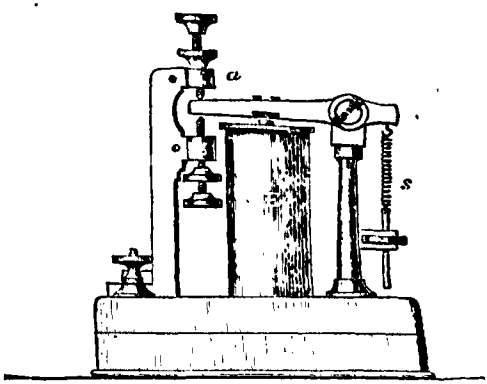


Fig. 1.

Here are several examples. Fig. 2 shows the construction and mode of working of one; and in order to show you the actual working of the telegraph, we have established at the opposite ends of this table two stations.

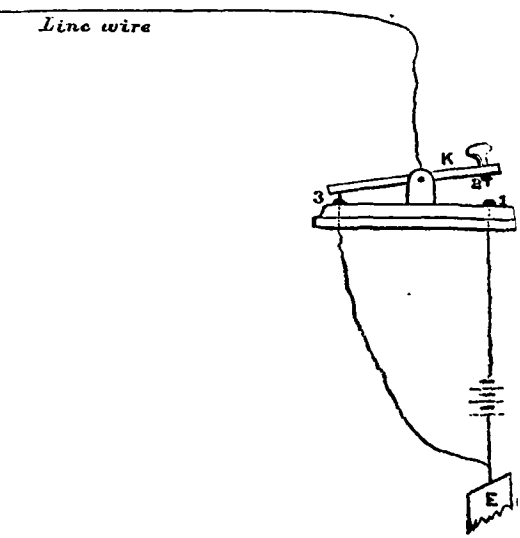


Fig. 2.

But, before referring to the stations, I must go back to one point, and that is, how can we convey these currents of

electricity from one station to another? This is simply done by wires of different kinds and characters. Here is a specimen of the ordinary galvanised iron wire that you see suspended from pole to pole along our railways, upon our roads, and over our housetops; and when rivers and streets and other obstructions are met with, we have to surmount them by placing the wires submarine or under ground; and here is a specimen of the gutta-percha wire used all through England for that purpose. Here, again, are specimens of submarine cables that are used not only to cross rivers and tunnels, but those two coils that you see at the back are two actual pieces of the Atlantic Cable which were raised from a depth of about 2000 fathoms. You will see by examining the ends of the cables that they are all made on precisely the same principle; a copper wire is surrounded by gutta percha, and in the case of our submarine wire it is still further protected by hemp, and outside by iron to protect it from the action of tides upon rocks, from the action of ships dragging their anchors, and from a multitude of causes which render the life of a submarine cable very precarious. You will find that these cables are constructed of an additional strength to meet cases of greater danger. For instance, in the North Sea, where ships frequently drop their anchors, the cables are made very heavy and very strong, but in deep water, where such dangers do not exist, no ironclad protection is used except when approaching the shore. Here we have a very interesting specimen of the very first cable which was ever submerged, and that is the one connecting England and France from Dover to Calais. This cable was submerged in the year 1851, and I believe that portions of it exist in working order to the present day. It is an interesting specimen, because in reality almost the very first attempt that was made to lay a cable was made with one of the same character, that has formed the type of every succeeding cable. It would seem that Mr. Crampton and those who assisted him in laying this first cable instinctively jumped to perfection without passing through those various stages of education, experiment, and growth which are found in other branches of telegraphy.

The great difficulty we have to contend with in the erection of telegraph lines is to prevent the possibility of the current which flows along

the wires seeking the earth and there becoming lost in immensity. The first wire, which was suspended on poles on the Great Western line, was insulated, as it is termed, with a simple quill, but it was speedily found that that was insufficient for the purpose, and little earthenware rings were next used to support the wires, then those were found insufficient, and a larger kind instead was employed; and you will perceive on this pole at the end of the room innumerable types of the forms that have been used at different periods. In some the wire is supported below the arm and in some above. They all mark a stage in the life of the insulator; each differs from the other probably in a point invisible to any one but a skilled telegraphist. But in each case we find that the progress of experience has been gradually attracting us as it were to that perfection, which however we have not yet reached, in the mode of supporting our insulators. In submarine cables gutta percha is the material we use to protect our wires from the water and the earth, and here is an interesting specimen of what we call the *fossil telegraph.* It was the first telegraph ever laid down in the world for practical purposes, and it was laid under the ground between Euston and Camden Town. Five wires were embedded in creosoted wood and the wires are simply protected by a coating of hemp. It was found totally insufficient for the purpose, and drove our engineers to the employing of iron wire suspended on poles.

Having thus shown you that we have batteries to produce our force; that we have sounders that enable us to impress the letters of the alphabet upon the mind; that we have wires suspended between towns and cities, upon poles, or carried beneath streets and rivers by means of gutta-percha, we have all the apparatus required to construct and carry on telegraphic communication between one point and another. We have here a station which I will call York, and at the other end of the table we have a station which we will call Southampton. Now, I am in York, and if Southampton wants to communicate with me, he must call my attention. Every town has a particular call, generally the initial letters of the town. For instance, York is YO, and Southampton SO, so that when Southampton wants to call me, he will

call me by giving the letters YO, which in reality become the christian and the surname of the station I am supposed to be at. When he has called my attention, I reply by simply saying, "York here." There is a clerk at that end of the wire, and there is a clerk at this end; everything is prepared for the transmission of the message from one place to the other. All that we want is the message, and I have asked our Chairman to be kind enough to write out a message which I will hand to Southampton to transmit to York for it to be written down. (The message having been sent accordingly.) This is the message:—"From Chairman to Lecturer. How Galvani would have rejoiced to foresee the development of his observations on frogs' "legs!" Now you see that we have there two stations properly arranged for the transmission of messages; there are only two stations, one at each end of the wire, but there might have been 3, 4, 6, or 10 stations on the same wire; and I have often been asked how is it, when there are several stations on the same wire, the particular station for which the message is intended takes off his message, and other stations do not get mixed up as it were with him. You must recollect that when several stations are connected together on one wire, to them space is absolutely annihilated. They are practically in the same room. Just in the same way as I am now addressing many hundreds, and every one of you can hear what I say, if you had been separated from each other by miles of wire, and I had communicated to you by that wire, you would as much have been in the same place as you are now in this room. Suppose we have five stations connected together called *a*, *b*, *c*, *d*, and *e*. If *a* wants *c*, he must simply call *c*, as you saw just now, and everybody else knows that *c* is being called, and *c* answers and everybody knows that *c* answers. Supposing I had four boys before me, Jack, Bill, Bob, and Harry. If I wanted Bob, Bob would answer and the others would not interfere; they would know I wanted Bob and they would be quite content. So with four stations on the same wire, if one is called the others know it, and the one that is called attends to his instrument and takes off his message, precisely

in the same way as that message was taken off just now. This is the reason why all stations have got their christian and surnames, or, as we call it, their *code*. Every station throughout this country has its code, and it is by means of these codes that they are called and principally known. The code being the combination of the first letters, as I pointed out in the case of York and Southampton. In the case of Liverpool it is LV, of Manchester MR, of London TS. Why TS? Because that happened to form the initial letters of Telegraph Street, where the central station was first formed. There are many Ln's, Ld's, and Lo's, so that TS was adopted as the code for London, and in the telegraphic world London is known very much better by the simple TS than by London.

Before I show you any further working I want to point out to you that inasmuch as I have been able, I hope, to explain to you that the letters of the alphabet can be formed by sounds, so they can be formed by means of the other senses. The same principle precisely which gives you long and short marks, through the ear, gives you, as you see by this diagram, the same marks by the eye. It can also be sent to the receiver by the organs of taste. I will not try the experiment before you, for I know its consequences, but if I were to put one end of this wire under my tongue, and the other end over my tongue, I should feel a very unpleasant indication that electricity was passing; but if I happen to be on a railway, or on an open road where the currents of electricity are not quite so powerful as those I am using to-night, then it would become perfectly possible to read what is passing by means of the tongue, which marks the various currents of long and short duration. There are many actual cases recorded where communication has been made in this way by means of the tongue. Broken-down trains have by that means communicated with stations, and line repairers have communicated with their inspectors. For the same reason it is equally possible to telegraph by means of the touch, because, practically, if I hold the wire in my hand I can feel the currents passing through me; and if I broke the wire connecting these two stations in two, and held each wire in my hand, I could feel

by my sense of touch that currents are passing. It is also possible to telegraph by means of smell. I do not know that it has been done, not at least within my own experience, but we are told, by no less an authority than an American paper, that on the occasion of the death of President Taylor the announcement was sent all over that vast continent by means of telegraph wires and by means of every sense. In most of the offices they read by sight; in many offices they read by sound; in others they read by means of the touch; and in others by means of the tongue; but this credible authority mentions that a blind girl succeeded in actually smelling the odour of ammonia which was developed by currents of electricity passing through a chemical compound.

Now, I will go to telegraphs dependent on sight. The first telegraphs introduced in England were telegraphs based on visible

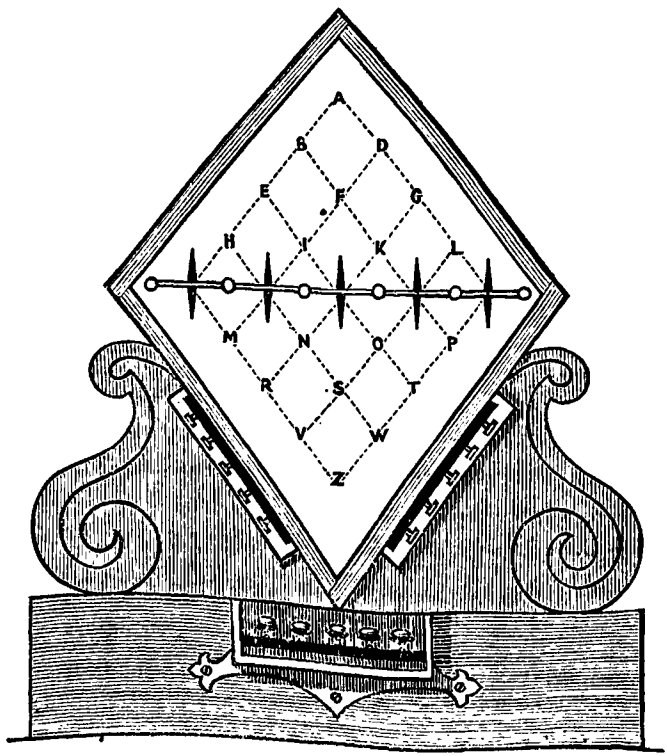


Fig. 3.

signals, and we have here the actual instrument which was designed by Cooke and Wheatstone in the year 1837.

That instrument was used for communicating between Camden and Euston through those very underground wires that formed what we now call the fossil telegraph. Here the letters of the alphabet are written upon this lozenge or hatchment-shaped instrument. Each needle on it has two movements, one to the right and one to the left, and when two of these needles are deflected, their convergence points to a letter, and by means of these five needles we are able to point directly to the actual letters of the alphabet. This instrument required five wires to work it, and it was speedily found that sufficient letters of the alphabet could be made with four instead of five; I say sufficient letters, because it is found that the English language can be written without the sound of c, q, or z; *c* always has the same sound as either *k* or *s*, but *q* is sometimes rather an awkward letter to signal. There was a great murder com-

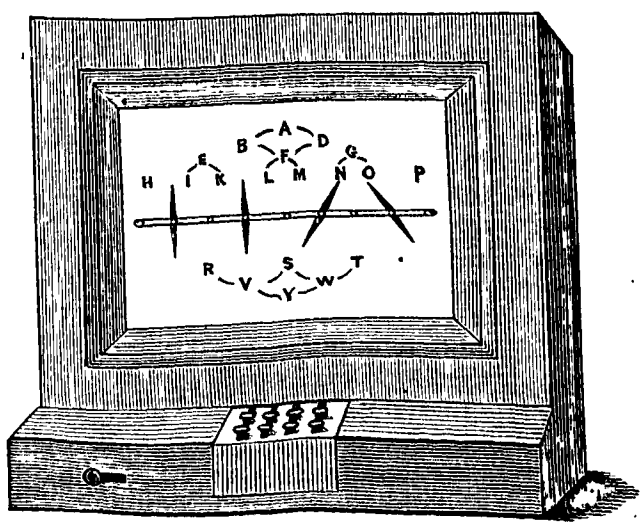


Fig. 4.

mitted some years ago at Slough, and one of the first purposes for which the telegraph was employed was to capture the murderer. The message was sent from Slough, and you will find it engraved on the instrument. I forget the exact words, but it was like this:

"A murder has been committed; the murderer is supposed to be a quaker." Now, "quaker" is a very difficult word to spell without the letter "q." It was spelt "kwaker," and the clerk at the Paddington end could not make out what the "k" meant, and the murderer was very nearly escaping in consequence. However, he was caught, and nothing tended so much to the notoriety of the telegraph as that capture of the murderer Tawell, in 1845.

This instrument was used on the Blackwall railway in 1840 and '42, and, by a mere accident, two brothers, stationed at different stations on that line, found that they could communicate with each other with only two needles, by making use instead of only one deflection to the right and one to the left, of two to the right and two to the left, or two to the right and one to the left, and varying them in that way. That at once led to the introduction of the double-needle instrument, which you see here.

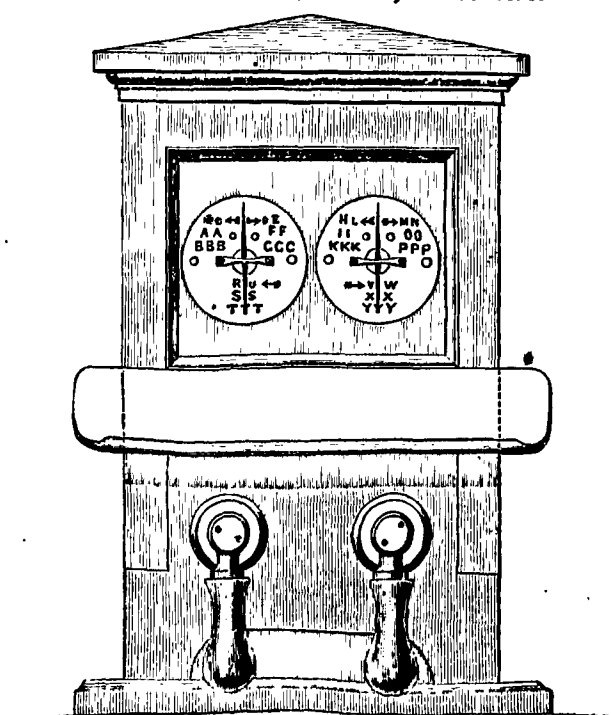


Fig. 5.

This instrument—the Double Needle, Fig. 5—is the darling of English telegraphists. It is the instrument by which they were educated; it is the instrument that was born and bred in England, but, unfortunately, its dearest friends are seeing it rapidly disappear, for the simple reason that it requires two wires to work it, and, therefore, it becomes expensive. But by dispensing with one wire, and by using precisely the same alphabet which I have depicted to you by sound, which is shown on this diagram by dots and dashes, the same alphabet can be formed upon this single needle instrument by making one movement to the left a dot, and one to the right a dash; so that by taking two instruments of this character, and fixing them up as I have done here, to represent two stations, I can send a message in the same way as before. Here you see that by precisely the same means we appeal to the eye, as in the previous case we appealed to the ear. We appeal to the eye in other ways.

The instruments I have shown you are what we call non-recording instruments. Their indications are simply transient—they disappear. But there are instruments which record upon paper these dots and dashes that you see on the diagram. Southampton, at the other end of the table, has an apparatus which thus records its messages; it is called the Morse apparatus, which shows the letters sent by means of these dots and dashes; and if our Chairman will be kind enough to write another short message, we will send it in the reverse direction, and we shall have it recorded at Southampton by this dot and dash alphabet. Each station is fitted up almost in the same way. We have a battery at each station to produce the force. We have a key at each station, to enable us to manipulate the currents, and to send them so as to make the letters of the alphabet; but, at one station, we have a little sounder to record the letters, while at the other station we have a Morse indicating apparatus to record the words. (The message was sent accordingly, but with some delay.)

There was a rather interesting operation performed before your eyes. Owing to some cause, the recording apparatus at Southampton had got out of order, and the clerk, finding the signals did not come properly, asks his colleague at this end of the wire

to "send v's." V is a very convenient letter; it requires a series of dots and dashes in such a way that they enable the clerk, by simply twisting a screw to adjust his instrument, so as to bring out his letters clearly. The interruption is remedied and we get this message:—"The world owes a vast economy of time to Wheatstone, the illustrious inventor of our system." The letters are all marked on this slip in ink, and, after the lecture, those who are interested in the matter will find complete alphabets worked off for them, which they will be welcome to take away. There are other methods of recording these letters; one is by chemical agency, and we have here one of Bain's chemical marking apparatus; but, inasmuch as the letters recorded by it are precisely identical with those recorded in ink, I will not delay you by attempting to describe it.

You will notice that in sending these messages the clerk has to manipulate the keys by means of his hand, and this is a tiring operation. One, who in the morning is able to send messages at the rate of 40 or 50 an hour, by the afternoon finds that his speed has been lowered about 20 per cent.; and as the day progresses, his speed becomes less and less. This in early days led Bain to conceive that it would be possible to send messages by automatic means. He conceived that if you took a strip of paper and punched holes or dots and dashes in it, that the paper so punched would send the dots and dashes, and would send them with greater regularity and with greater speed, and without all the defects of the human machine. It was tried and found successful; but in those days it was not wanted, and it dropped. In later days Sir Charles Wheatstone brought that wonderful genius of his to bear upon this question. The time had arrived when the automatic telegraph had become necessary; messages increased; the demands of the newspapers increased; and the time had come when automatic telegraphy was a necessity of the age. Bain's idea of punching was adopted by Sir Charles Wheatstone, but it was so altered and improved that a totally different apparatus was brought to light. Strips of paper, such as I hold in my hand, are punched with a number of holes by an apparatus which we have here; and this punched paper is placed

in an apparatus called the transmitter, and which in reality is the part of the apparatus that replaces the hand. Instead of requiring time to be divided into these dots, dashes, and spaces which form the alphabet by hand, this is done for us by means of the machine, and the result is that we are able to get the greatest possible speed out of our wires. While a clerk can only send at the utmost about 40 words per minute, there is no reason why this instrument should not send at the rate of 200 words per minute. In actual practice, we do not send more than 130 words a minute, but that is due to electrical causes existing on the line, independent of the principle of the apparatus itself. Instead of having this transmitter separated from its corresponding receiver, as we have in the other instruments, and the key separated from its corresponding recorder by the length of the table, I have brought these two instruments close to each other, and by an ingenious contrivance, I simply take the alphabet as it is punched, and fixing it together, we are able to make this slip of paper go round and round, and so record as many Morse alphabets as we please. This system is the one most extensively used in England. This Wheatstone automatic apparatus is used to all our great centres of commerce, and to all our stations for press purposes, and it is one of the most valuable aids which telegraphic science has received within the last ten years. Its inventor has, unfortunately, departed, but not before he saw the fruits of his great genius.

I have only time to call your attention to two other methods of telegraphy very extensively employed. The first is where the letters of the alphabet are indicated themselves in unmistakable letters, by that which is called Wheatstone's A. B. C. Apparatus. (Fig. 6.) It communicates with another similar apparatus at the end of the table. The other instrument is probably the most ingenious that we have; it is called Hughes' Type Printer. The letters of the alphabet are printed by it in bold Roman type on a strip of paper, and handed to the receiver as they are rolled off the instrument. We will set the instrument going and send a few words by its means.

There are other methods which I cannot stop to speak of;

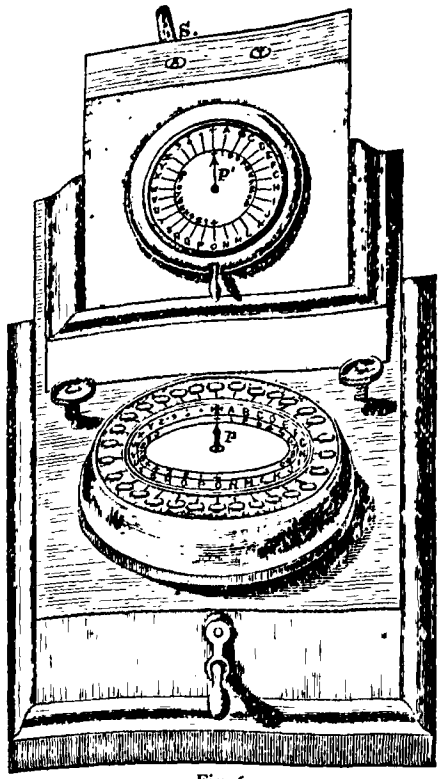


Fig. 6.

amongst others, there is the Duplex Telegraph, giving rapidity of signalling which the tendency of the age is daily forcing upon us ; and whilst on the one hand we are endeavouring with all our might to reduce telegraphy to its greatest simplicity, we are, on the other hand, compelled to meet the requirements of the commercial classes by introducing telegraphy of great rapidity, of great capacity, and, in consequence, of considerable complication. We have in this room examples of the two extremes, the simplest possible apparatus and the most complicated that we have in use at the present day. Here is our one station, with a simple key and a simple little sounder, which is simplicity itself; and on the table before us we have an apparatus which is used for telegraphy be-

tween distant stations, such as London and Cork. London and Cork cannot communicate with each other at a very great speed unless we insert at the middle point, Haverfordwest, an apparatus similar to that which I have here, which is called a translator; and this translator is, without exception, one of the most complicated pieces of apparatus that is used in the telegraphic world; but at the same time it is of such perfection in its operation that, by inserting such an apparatus at Haverfordwest, between London and Cork, we are able to increase the capacity of wires connecting those two places by from 25 to 30 per cent.; that is, instead of being able to send about 70 or 80 messages an hour, we are now enabled to send about 100 or 110.

Telegraphy on a table like this, and before an audience, is comparatively a simple matter, but when we come to actual practice we meet with disturbances of various kinds. A great enemy of ours is lightning, which plays sad havoc with our instruments. Sometimes a needle instrument is burst to pieces as though an explosion—not as though, for it is an explosion—had taken place inside it. These delicate apparatuses are fused, and burnt, and destroyed, although we protect them as much as we possibly can with lightning protectors, but we cannot always prevent the lightning committing damage. Sometimes it also destroys our poles, and here is the top of one which was shattered by lightning. We have even found the iron wire fused. Here is a specimen of iron wire fused into about a dozen different pieces by a flash of lightning.

And so I could multiply cases where lightning has done damage to a considerable extent, but I must pass on to other causes of disturbance. Our wires are sometimes destroyed by Railway accidents; they are damaged by storms; they are broken down by snow, and all these moving accidents by flood and field attack us and injure us at all our vulnerable points. Sometimes wires are brought in contact with each other, and curious cases occur. For instance, during one of the snowstorms we received a considerable damage in London, where we have, in addition to the public wires connecting the stations, private wires. On one occasion after one of those snowstorms two private wires were

brought into contact; one belonged to a shipping firm and the other to some other commercial house. I do not remember what. The shipping firm was very anxious to know of the safe arrival of a ship which was overdue at Gravesend, and on going to the office in the morning the chief of the firm heard the needle move, and at once went to his instrument, and without "calling," as you saw just now, asked the question, "Is anything known of the 'Earl of Devon'?" The answer he got back was, "Go to Bath." Being naturally very indignant at the head of the firm being treated in such a way, he repeated his question, "Is anything known of the 'Earl of Devon'?" and got back the reply, "Yes, he has just gone to dinner in his stage coach." There is a receiving house in the City that some of you may know, called 'the Swan with two necks.' That has a private wire, which got into contact with another private wire. The owner of the other wire saw his instrument move, went to it, and asked "Who are you?" The reply was, "Swan with two necks." He said, "If you were a swan with a dozen necks you would be of no use to me." Of course every precaution is taken to prevent such accidents, and when you know the innumerable vicissitudes to which wires are liable, you would be surprised that they are so few. A frequent source of trouble in the south of England has been birds. I have known when the swallows are preparing for their homeward flight contacts frequently occur by swarms of those birds settling upon our wires, and then flying off with one mind, and so setting them all vibrating until they came in contact. At one time we found it necessary to provide against this by keeping boys to frighten away the birds as farmers do in their wheat fields, but we got over the difficulty by putting the poles so near each other as to prevent this vibration. We have had two or three cases of a wire being broken by a goose flying against it; of course the goose came to an untimely end, but it also broke the wire. I have pieces of wire in my possession that were broken in the neighbourhood of Guildford in that way. Another source of trouble in gutta-percha wires in our offices is due to rats. They frequently make holes through the gutta-percha; they do not seem to enjoy the gutta-percha much, they simply gnaw a little hole

in it, and if perchance their noses touch the wire and they experience our acoustic system, they draw back astonished and probably disappear, but at the same time even the smallest hole gnawed by a rat in a wire will frequently, by the action of the current itself, eventually produce a complete collapse. I have known instruments too suffering from animals in peculiar ways. In one instance one of those single needle instruments was broken by a kitten playing with it; and I came upon the following curious entry in an old diary belonging to a station in Persia, "Cannot adjust relay; scorpion on top of relay barking like a dog."

As you are all aware, the telegraph is not inviolable; errors are made, and when you see by how little one letter is separated from another, it is not to be wondered at that mistakes sometimes occur. On the old double needle instrument, for instance, the letter x and the letter y are very similar and are sometimes mistaken for each other. A person telegraphed to a station-master that he had left a black box in a carriage; he got a reply back, "I cannot find the black boy anywhere." A lawyer had forgotten to take with him his wig, and he sent a message back home which was delivered, "Send me my wife in a band-box." Another individual was going to be presented at Court, and had forgotten his cocked hat; the message received by his servant was, "Send me my cooked ham." All these errors are easily accounted for, when the letters of the alphabet are hastily or incorrectly sent. I have on this table put several words which are easily mistaken for each other, "all" and "fall," "save" and "have," etc.; and you can easily see that when a man sent a message, "Send hack to meet me at station," the message was sent, "Send a sack to meet me at the station." There is another very strange source of error in telegraphy, which is one worthy of the examination of psychologists; it is a purely mental error. A person reads a message and absolutely conveys the distinctly opposite meaning. For instance, there are two or three well-authenticated cases where the clerk had distinctly before him the word "Yes" and sent it "No." I saw myself a message which announced the fact that "Edgar had passed." Edgar had probably been through some competitive examination, and the message

sent was distinctly, "Edgar has passed." The message received was, "Edgar has failed." Now these cases are fortunately extremely few; but there is no doubt that between the time that a clerk reads the word upon his form and the time that he sends it upon his instrument, there is a mental operation that passes in his mind which exactly reverses the meaning of the word he wants to send.

Another source of error is due to a system which we are obliged to introduce, by which words are taken down by a species of shorthand writing. In press messages and long messages, a system of abbreviation based very much on the shorthand system is used; but there are certain conventionalities adopted between clerks themselves that are very curious; for instance, the word "children" when frequently occurring is indicated by k–ds, meaning "kids." One of our leading politicians delivered a speech on the educational question to a meeting in another part of the country, and he frequently spoke of "the religious education of our children." This was sent—"kids," and thus appeared in our leading journal, sometimes repeated in inverted commas "the religious education of our 'kids.'" Of course the member of Parliament was naturally irate at the insertion of such grotesque vulgarisms in his speech. An explanation was given of how it arose, and of course the opportunity was taken to forbid or stop a system of abbreviation based on such an erroneous principle. Another very curious mistake was made in a speech of no less a person than one of our archbishops, I am not sure which. In a speech he is reported to have said, "The Post Office Telegraphs were the best exposition of the opinions of the age." The "Post Office Telegraphs" had been signalled instead of "the Poet Laureate." I could multiply these cases to any extent, but I merely wish to point out to you how such errors are produced, because although they occur I don't think that any great fault can be attributed to those who make them. You must see, from what I have shown you, that any system of telegraphy based upon a system, the accuracy of working of which depends entirely upon uninterrupted attention, upon closely applied skill, and upon great talent, must be liable to occasional errors; and if a com-

parison were made between the percentage of errors and the enormous number of messages that are sent, great credit must be given to the telegraph clerks of this country who make so few. Of course when an error of this kind is made, it is at once made the most of. The person who suffers from the error is naturally loud in his complaints, and naturally therefore errors attract a great deal of attention. I am not sure what the present percentage is, but a short time ago it was not one error in ten thousand messages, and my impression is that the proportion has very much decreased. At the present time we are unfortunately not in a position to obtain an accurate account of the ratio that exists, but at any rate the percentage of errors is extremely small.

Many of you have probably from some distant point observed some grand old battlement or castle covered and coated with creeping ivy. If it were possible for us to transport ourselves to some distant place, and survey this grand old globe of ours, we should find it similarly enclosed with a network of wires. In the case of the old castle, centuries elapsed before this creeper enveloped its ruins, but in the case of this world it has been enveloped during our own lifetime. Thirty years only have passed from the time when commercial telegraphy first took its start to the present day; and when I first commenced my telegraph career, there were fewer messages sent in a year throughout the whole of the world than are now sent in one day through the Postal Telegraph service. That battlement to which I alluded tells tales of the horrors of war, but this network of ours tells a tale of the blessings of peace. The greatest civiliser in the world is the telegraph. But for our wires, the probability is that now we might have been at war with Russia. If we had had wires in the days of the mutiny in India, how soon would that mutiny have been quelled? It stopped, probably, differences between England and America, and its great tendency is by bringing together into one space all the families of the globe to produce peace and harmony.

The CHAIRMAN: There remains little for us but to return thanks to Mr. Preece for his admirable lecture. It is rather astonishing to several of us, as I see few faces as old as myself

present, that we have lived through three of the discoveries without which life would seem now almost unendurable. When I was young there was no means of getting instantaneous light except the tinder box; there was no means of getting from one part of England to the other except the stage coach; and there was no means of communicating your thoughts except through the medium of the Post Office. Now all this has been changed, and the most remarkable of the changes is the telegraph. Just consider what a difference it has produced! The most remarkable case of speed which is recorded by our ancestors was after the death of Queen Elizabeth, when the sister of one of the courtiers threw out a signet ring to show that she had died. The courtier galloped off with relays of horses to Edinburgh to tell James he was King of England, and he managed by wonderful exertions to do that in less than three days. Now, a message could have been communicated long before he could have saddled his horse. The economy of time is perfectly wonderful. The "tricksy Ariel girdling the globe" is nothing compared with the speed of the telegraph; and what is the most remarkable thing, which perhaps does not strike us all at once, is the wonderful economy of time it produces to a nation. Just for an instant try and realise what it is with regard to England! All commercial and manufacturing men say that the telegraph saves about half a day's time in the economy of production. Now there are twenty millions of telegrams sent annually in this country, which represent ten millions of days in economy of time. Divide that by 365, and find out how many years of economy is compressed into a single year in this country. No less than 27,000 years are compressed into a single year. About three or four times the time occupied in the history of the world as ordinarily regarded, we get in economy of time concentrated into a single year by this wonderful communication. Therefore it is very interesting that we have in this exhibition such an excellent display as there is of all these modern instruments, and of those discoveries which have been developed in such a few years, and no one is better fitted than our Lecturer to explain them to a general audience. I am sure that you will agree with me that we ought to return our hearty thanks to Mr. Preece for his very interesting lecture.

PHOTOGRAPHIC PRINTING PROCESSES.

By Capt. Abney, R.E., F.R.S.

Saturday, June 24th, 1876.

The Chair was taken by Mr. Wm. Spottiswoode, Treas. R.S.

The Chairman: I will not detain the meeting by any lengthy introduction, but will at once call upon Captain Abney for the lecture which he has been kind enough to promise us this evening. It is doubtless well known to you that Captain Abney is not only a distinguished officer of the British Army, but he is equally distinguished in the arts of peace and science. He was recently in charge of one of the many parties sent from this country to observe the transit of Venus. And on this service his own observations have been of the highest importance, and are calculated to bear valuable fruits in the growth of science. I will, therefore, without further introduction, beg to introduce to you Captain Abney.

Captain Abney: Ladies and gentlemen, it is not my intention to enter into the history of any of the processes to which I propose to call your attention to-night, as I somewhat dread to enter upon such controversial ground. Probably the demonstration of the production of photographic prints by various methods will be of greater interest than any history.

Astronomy was the religion of the world's infancy; and it can hardly be a matter of surprise that untutored yet inquiring minds, unaided by any distinct revelation, should have attributed to the glorious orb the centre of our solar system the possession of divine attributes, and as they gazed upon the wondrous effects of his magical painting, that they should have offered to him their adoration and worship, and carefully noted any phenomena due to him. Thus probably the first photographic action noticed

would be at a very early period of human existence, when it was found that the exposure of the epidermis to his rays caused what is known to us as tan. For instance, if any of you have a ring on your finger, and you do not wear gloves, you soon find when the days are bright the skin will become tanned, and the skin underneath your ring will be perfectly white.

Another photographic action which would be remarked at a later date would be the fading of colours in the sunlight. Ribbons, silks, cottons, and similar fabrics of a coloured nature, undergo a change in tint when exposed to it, and it is no uncommon thing for a correct likeness of a bow to be imprinted by the light on a ribbon beneath it. I have here a specimen of a pink trimming used by the fair sex; and the lady who presented me with it told me that the colour went after two days' wear in the sun. I thought this a capital opportunity for trying to get a photographic image on such a quickly fading material, and I have here two of the results. The first was exposed under a negative of an anatomical subject for twelve hours in the sun, and on examining it you will find that the parts exposed to the light are quite white, and we have the picture of the subject represented as white on a pink ground. The other subject is a map. The pink stuff, with a map placed above it, was exposed to the full action of the sun for twelve hours, and we have a representation of the map in pink lines on a whitish ground. This pink colour, we may say, is impressionable to all sunlight, and the action of that light is visible when the fabric has been exposed for about twelve hours. Mauve ribbons, as some ladies know, will fade in an equally short time; and had I had more time I have no doubt I should have been able to give you specimens of prints in every colour. The general idea amongst the fair sex seems to be that the colours are given off somewhat in the same way that scent is given off from the rose, but I am afraid we cannot endorse that opinion, because if it were so, they would fade whether exposed to the light or not. There are other bodies also with which we are familiar, which are sensitive to light. I will take, as an example, common glass. My friend Mr. Dallmeyer has some specimens of

glass, some flint and some crown, which were exposed during two or three years to action of light; half being covered up and half being exposed to the action of the sun's rays. That part of the flint glass which has been exposed to the action of the sun's rays has turned a yellow colour, and that part of the crown glass which has been exposed to the sun has turned a purple colour. Therefore you see substances which we should think most unlikely to change, will change.

The bodies which change rapidly in the light, however, are those which are really the most useful in photography. The best and most prized amongst these are the compounds of silver, for they are peculiarly sensitive. Nearly every salt of silver is more or less changed by light, and when I say changed, I mean altered in composition. When we come to consider what light really is, we can better understand its action. Light, as experiment, confirmed by mathematical analysis, tells us, is caused by a series of waves issuing from the luminous source, not trembling indeed in our atmosphere, but in a subtler and infinitely less dense medium which pervades all space—known as the luminiferous ether. The waves batter against any substance exposed to their force, a good many millions of them striking it every second; and surely it is not surprising to think that, small as these waves are, yet that their persistent battering should in some instances be able to effect a decomposition. Take as a type that salt of silver which was perhaps the salt of silver first known to change in the presence of light—chloride of silver. We may represent chloride of silver, for our purpose, as made up of two atoms of silver locked up with two atoms of chlorine. Now, when light acts on these four atoms, some of the waves encounter a resistance to their swing. They beat against them so energeticaly and persistently that they liberate one of the atoms of chlorine, and leave the two of silver and the one of chlorine behind, forming a dark bluish-brown compound known as the subchloride of silver. I will get Quartermaster-Sergeant Doyle to expose some of the chloride of silver to the action of the light produced by magnesium wire, and you will see that a blackening ensues. Suppose I place a piece of paper impregnated with

chloride of silver beneath a body partially transparent and partially opaque (such as this piece of glass with black lines upon it), and then place it in the light, I shall get an image of the lines in the white chloride of silver, and an image of the transparent parts in the dark subchloride of silver. Of course, if I removed the paper from beneath the glass, the chloride of silver composing the white lines would blacken in the light, and the image would disappear. Fortunately for photography it was discovered by Sir John Herschel that the chloride of silver was dissolved by a salt known as sodium hyposulphite, so that by applying a solution of this salt, the sensitive salt of silver was dissolved away. It also attacks the subchloride of silver, taking away one atom of silver and one atom of chlorine from the two atoms of silver and one of chlorine, and leaves one atom of metallic silver behind. Now, the action of light upon this chloride of silver can be very well demonstrated, as a laboratory experiment, if we take pure chloride of silver in a test tube in which there is a little distilled water, and expose it to the action of light. The silver salt will blacken; and if you analyse the water afterwards and apply the proper tests, you will find the presence of chlorine in the water, showing that the chloride of silver has been decomposed into the subchloride and chlorine. In our ordinary silver prints we have other bodies present in the paper besides the chloride of silver. I have here a body known as albumen which is found in the white of egg. If I take this albumen and drop into it some nitrate of silver, I shall get albuminate of silver formed, which is a white curdy precipitate and is also sensitive to the action of light. If then paper be floated on a solution of albumen, which has been previously impregnated with chloride of sodium, and then, when dry, be floated on a solution of nitrate of silver, we shall get, by what is known to chemists as double decomposition, chloride of silver and albuminate of silver upon the surface of the paper, and this when dry is ready for printing. I have here a picture which has been printed in this way on albumenised paper. A coating of albuminate and chloride of silver has been given to it, and it has been exposed to the light. If I immerse this in a solution of hyposulphite of soda, we shall find that the colour becomes very dis-

agreeable; it is a foxy red which is unpleasant to the sight and would be called inartistic. In order to get over that difficulty, photographers have had resort to a method of gilding the silver image by immersing the print in a neutral solution of chlorine of gold. The gold precipitates upon the silver, and we get an image formed in silver and gold. Gold when precipitated in a very fine state is not of the yellow colour with which we are ordinarily accustomed to associate it, but of a bluish-green tint, and the effect of the minute particles of gold when precipitated from a solution of chloride of gold is to colour the silver; the red colour of the silver oxide and the bluish-green of the gold mix together and give us an agreeable tone, such as we have here. One portion of our picture has been immersed in the toning bath and the other has not. Both have been immersed in the hyposulphite of soda solution. You see the difference in colour between the two.

I dare say some of you may recollect that a few years ago there was a great demand for certain photographs which were called magic photographs. They were originally, I believe, supplied in packets containing directions how to produce them; and it was usually stated that a piece of the blotting-paper which was enclosed in the packet was to be damped, applied to another piece of paper on which a photograph was to appear, and that after such application an image would start out. I will endeavour to show you how those magic photographs were prepared. Here I have a print which has been washed and immersed in a bath of hyposulphite of soda, and I am now going to plunge it into a solution of chloride of mercury. I gradually find that the image begins to fade. What has happened? The chloride of mercury combines with the silver to form a white salt which is indistinguishable from the surface of the paper. I have one here which has been treated with mercury. I will not detain you by washing it, but will show you the print when it has faded away. Here is one which has been printed, washed, fixed, and immersed in the chloride of mercury, and if now I place it in the hyposulphite of soda you will see that an image begins to appear almost instantaneously. On the apparently blank piece of paper an image gradually developes, and we have before us one of these magic photographs. I need hardly say

that the piece of blotting-paper alluded to had been impregnated with hyposulphite of soda, and all you did was to do what I have been doing now; you brought the salt of mercury and silver which formed the image in contact with hyposulphite of soda, and a new black compound of silver and mercury resulted. I might wash this and place it in the mercury again, and it would again disappear, so you see that the magic photograph would last a great number of times.

Besides silver there are a great many other metals or metallic salts which are sensitive to light. I have here some paper which has been coated with perchloride of iron, as it used to be called, in which two atoms of iron combine with three of chlorine. The waves of light act energetically on this compound of iron when in contact with organic matter, the vibrations split up the salt and reduce it to what is called ferrous chloride, in which one atom of iron combines with only one atom of chlorine. Ferrous chloride, or ordinary muriate of iron, when brought in contact with a solution of ferridcyanide of potassium immediately strikes a beautiful blue colour. Over this piece of paper during the day I placed a negative of a map, and exposed it to the light for about five minutes. Now, if I float it upon a solution of ferridcyanide of potassium we shall at once get a beautiful blue colour, owing to the combination between the ferrous chloride and the ferridcyanide of potassium. The ground ought to remain tolerably white, because the ferric chloride has no considerable action on the ferridcyanide. You see at once we get the blue colour formed; in other words, an image is formed of the blue salt of iron. I have some other prints here which are better, for the light has been so uncertain to-day I hardly knew what exposure to give. The same iron salt, chloride of iron—and you must distinguish between chloride and perchloride—the chloride of iron has the faculty of reducing silver owing to certain chemical reactions. I have here a solution of nitrate of silver, and if I place this print, which has been exposed beneath a negative —the conservatory of one of the Windsor Lodges—in a solution of nitrate of silver, the silver will be reduced by the chloride of iron, whereas the perchloride would not do so, and we shall get a print in metallic silver. Simply washing is generally quite suffi-

cient to fix them. In the Exhibition there are some interesting specimens of the early applications of iron, to which I shall allude presently. It was Sir John Herschel who was the principal investigator into the action of light on these compounds of iron.

Uranium salts are also acted upon by light, and I propose to show you the method of producing prints by the action of those salts. I have here a piece of paper which has been brushed over with a solution of nitrate of uranium, and the salt which is reduced from the nitrate of uranium on the paper will combine in the same way as iron does with the ferrocyanide of potassium. Immersing it in that solution, I shall get a sort of dusky red print resulting. This picture was exposed during to-day, and I am not certain about the results, still it is developing; but I will pass round a print produced yesterday by the same salts. If I treat uranium in the other way in which I treated the iron salt, I dare say we shall get the same kind of result, that the oxide of uranium in the print will reduce the silver nitrate. I will place it in the silver and leave it to develope. You see it is developing. Here is a specimen which was prepared beforehand by the same process.

The latest metallic compounds which are known to be capable of being affected by light are the Vanadium compounds, which have been so thoroughly investigated by Professor Roscoe, and in the Exhibition you will find the first prints which have been produced by their aid. With iron salts also we have another most valuable process lately introduced, and worked out most thoroughly by Mr. Willis. He sensitized a piece of paper with a compound of potassium chloride and platinum chloride, and to it he also added a solution of oxalate of iron. I have here a solution of the potassium and platinum salt, which is a bright red colour, and the oxalate of iron is a pale yellow colour. These solutions being mixed together, he floats a piece of ordinary paper upon them, dries and exposes it beneath a negative, then immerses it in a solution of oxalate of potash and he finds the oxalate of potash in the presence of the lower salt of iron is capable of reducing the platinum to the metallic state. I have here a print which he has kindly sent me to develope. I may premise that the paper has previously been floated over a very dilute

solution of nitrate of silver, before the application of the salts of potassium and platinum, together with the oxalate of iron (which is the sensitive salt) and has since then been exposed beneath this negative. You see there is a slight blackening, due to the nitrate of silver, which has been reduced to the metallic state by the action of the light. I now place it on a warm solution of potassic oxalate; an image at once starts out, and we have a perfect picture resulting. That picture is formed principally of platinum black, a substance which is perfectly unacted upon by ordinary treatment. Of course it is soluble in nitro-muriatic acid, but in nothing else. I will send round a print which has been developed this afternoon by myself. The print is fixed in oxalate of potash, which will dissolve away the unaltered oxalate of iron after a slight immersion in hyposulphite of soda (one minute is quite sufficient to dissolve away the silver), and after washing in plain water for a short time we get a permanent print.

The next salt to which I will call attention is bichromate of potash. It is now largely used for all purposes of photo-mechanical printing, and permanent printing. This salt very readily parts with its oxygen in the presence of light; organic matter is oxidised by it, and the bichromate of potash becomes reduced to a salt of potash and oxide of chromium. To show you that there is a change produced by light upon the bichromate of potash, I have here a piece of paper which has been impregnated with it. If I immerse this in a solution of nitrate of silver, I shall find where the light acted no perceptible change takes place; but on all other parts we shall get the bichromate of silver. The print is developed at once, and we have buff-lines upon a brilliant brick coloured ground.

Whilst treating of this process I should like to bring under your notice another one which I cannot unfortunately demonstrate to you to-night, which is called Willis's aniline process, perhaps one of the most beautiful with which I am acquainted, particularly for architects and engineers' work, such, for instance, as reproducing plans. A paper is floated on bichromate of potash, and a little feeble acid is added to it, which is called phosphoric acid. When dry this paper is placed under a plan—not a nega-

tive, but a plan or a drawing, in a printing frame, and instead of darkening, the bichromate bleaches to a certain extent. Immediately on withdrawing this paper which has been acted upon by light from the frame, fumes of aniline are allowed to play upon it; in practice the aniline is dissolved in a certain quantity of alcohol, and almost immediately the phosphoric acid in the presence of the bichromate of potash forms a black colour on the parts not exposed to the light. It is insoluble, and when washed with acidulated water and then with pure water, we have a comparatively permanent print. The principle of obtaining this black print is familiar to those who know the working of the aniline dyes. This compound is, I believe, the residue left after the formation of aniline purple, according to Mr. Perkin's method.

Another process which has become very familiar to us all, is that known as the carbon process. That is perhaps rather a misnomer at the present time, although it was not at first, as carbon used to be mixed with the gelatine. It is now best known as the autotype process, and is dependent on the following chemical reaction. If gelatine be soaked with bichromate of potash, allowed to dry, and then exposed to light, the gelatine becomes insoluble; it is oxidised, in other words, at the expense of bichromate of potash, and only that part which has not been acted upon by light is capable of being dissolved in water. This paper has been coated with gelatine and bichromate of potash, a square piece of black paper has been placed on the centre of it, and it has then been exposed to the light, and the bichromated gelatine has become discoloured owing to the formation of the oxide of chromium. When we immerse this in water you will find the gelatine will dissolve away, leaving the protected part of the paper white and the outside fawn-coloured, owing to the oxide of chromium present. Now, if we have gelatine impregnated with any pigment and bichromate of potash, such as we have in a sheet of autotype paper, and this is exposed to the action of light under a negative, the part which is acted upon by light becomes insoluble to a depth depending on the intensity of light

passing through the negative, whilst the other part remains soluble. If I had a print on such a piece of paper printed beneath what we call a half-tint negative, that is, a negative with shades in it, and immersed it in hot water, the result would be most unsatisfactory; for this reason, that the soluble gelatine is really imprisoned between the back of the paper, and the insoluble gelatine. If I immerse it in cold water I shall get no result, but simply a slight swelling of the gelatine. To overcome that difficulty resort has been had to a method of taking the paper away from the back of the gelatine, and this is done by atmospheric pressure. The paper is immersed in water with a zinc or porcelain plate underneath, and a fine layer of water is kept between them; the plate, with the paper upon it, is raised up from the water, and with a squeegee the water is pressed out from between them, and the gelatine is thus caused to adhere to the surface of the zinc by atmospheric pressure. I have one sheet which has been exposed, and by this treatment you see how the gelatine adheres to the surface of the plate. We will now have it immersed in hot water, when the paper will strip off. In this case we have soluble gelatine on the front of the picture, the insoluble gelatine forming the picture remaining behind on the zinc plate. I have here a picture with the paper stripped off partially developed. If I immerse it in a trough of hot water, the remainder will develope, and we shall get a picture on a temporary support, in the form which we have here. All the unaltered gelatine dissolves away, leaving the altered gelatine containing the pigment behind to form the gelatine print. It is not very satisfactory to have a picture mounted on a plate of zinc or of opal glass, or on other supports known as temporary flexible supports, and therefore recourse is had to a plan of taking off the gelatine once more from the surface of the plate. Another piece of paper coated with gelatine rendered insoluble in water by the addition of a substance to which I shall refer presently, is moistened in hot water, caused to become sticky, and then this is squeegeed down on the surface of the carbon print. It is allowed to dry, and then the print is ready for stripping off. When this operation is finished the carbon picture is complete.

The next process to which I call attention is the Woodbury-type process, and a most valuable process it is. In this frame is exhibited the whole process from the beginning to the end except the mere press work. Here we have an ordinary negative, that of an Egyptian temple. A thin layer of gelatine is prepared on glass; it is made sensitive by bichromate of potash as before. It is then stripped off the glass and placed beneath the negative; the light goes through the negative, and the consequence is that on applying hot water to it from the back we get an image in relief, I might say in very considerable relief. This is developed on a very fine collodion backing, instead of on the plate, as in the autotype process. The gelatine film when dried is taken and placed on a soft metal plate, and as in nature-printing, so with this, a tremendous pressure is brought to bear upon it by means of an hydraulic press. The gelatine relief is impressed upon the soft metal plate and leaves a mould. Such a mould we have here, and I will ask this gentleman to kindly show you how the printing is done. In this bottle is a solution of gelatine with certain pigments, a small quantity of which is poured on a mould, which has been placed on a press. A piece of smooth paper is put over the gelatine, and a flat glass plate is brought down on the surface of the mould. The excess of gelatine is squeezed out from between the mould and the paper, and after a while the gelatine sets like a jelly, the paper is lifted up from the mould with the print adhering to it. These pictures then are formed in the same way as a mould of jelly is formed. The temperature of this room is rather warm, and it will take about three minutes to set. If you have a dozen presses fixed on a revolving table, you may have a dozen of these moulds going; you pour the gelatine, place the piece of paper over it, shut down the press, and then turn round the table a little, so as to get another press opposite you, and so on, and by the time ten or twelve moulds have been filled, the first will be set, and the print can be taken off. By this means thousands of prints can be produced in a day. If you look at the prints by reflected light you will see there is a certain relief about them. Prints produced by this process are as perma-

nent as the gelatine, and may be rendered more permanent by being placed after printing in a bath of alum. Most of you are familar with the fact that when you place a hide in tannin it becomes leather, and in the same way, when you place gelatine in alum it becomes like leather and insoluble in water, so that you have nothing to fear for these prints even if a drop of water does by chance get upon them.

The last process to which I shall call your attention is one of a different class altogether. It is one of a many photo-mechanical printing processes, and when I say photo-mechanical I mean a process by which, after the exposure of a sensitive layer to light beneath a negative, prints are produced by mechanical means. The Woodbury-type process is a photo-mechanical process, because, after one impression is taken upon the gelatine skin, it can be worked without the aid of light. In the same way with the heliotype process, which I now propose to describe. Of course it is a very great advantage for book illustrations, and so on, that you should not be dependent on the action of light for the production of each print. I will describe this process because it is one familiar to myself, since we work it at Chatham, and have done so for a considerable time, and we obtain very fair results by it. In none of the photo-mechanical processes have we yet arrived at perfection, but we look forward to the time when we shall be able to set up in the printing press a half-tone block, which shall be capable of being printed with the type. We have a promise of such a process in what is known as the Dallas tint, but as that is a secret process I cannot go into the details of it. There are specimens of it on the table.

In the heliotype process there are several operations: first, there is the preparation of the gelatine film on which the image is to be printed. A large glass plate, by preference ground, is placed on a bench accurately levelled, and a solution of gelatine, to which is added bichromate of potash together with a little chrome alum. It is floated over the plate in a pool and allowed to set; when set it is reared up in a slightly warm dark room, and allowed to dry spontaneously. After drying, it is carefully stripped off the plate, and you get as the result a tough skin, such as I have here. The

chrome alum was added to render it insoluble in water, and, of course, the bichromate was added to render it sensitive to light. Besides the fact that light renders gelatine insoluble, it has also the property of rendering it incapable of absorbing water, and this is a most important point, in fact *the* one on which most of this class of photo-mechanical processes is based. If chrome alum were not added, on immersion in water, of course the part unacted upon by light might dissolve away; but the chrome alum being added prevents it dissolving, yet at the same time allows the gelatine to absorb moisture. If this gelatine skin be placed under a negative and the light be allowed to pass through it, we shall get a distinct impression on the gelatine sheet. After it is printed, suppose you take that gelatine skin and immerse it in water, and place beneath it a zinc plate or a pewter plate or a glass plate, and take up a film of water between the gelatine skin and the plate—which, by the way, is generally coated with an adhesive substance, such as india-rubber — and then squeegee it down in the same way as we did the autotype tissue, the gelatine film will adhere to the plate. To prevent the water from getting under the edges of the gelatine, it is generally coated round with a solution of india-rubber, and gummed down with paper. You afterwards immerse the plate with the skin on it in water, in order to soak out the bichromate of potash. What happens while this bichromate is dissolving out? Where the light has acted, there it refuses to absorb water. Where the light has partially acted, it partially absorbs water; and where the light has not acted at all, it absorbs water entirely. Now we come to the same principle as is used in lithography. Water repels grease. Therefore, if you charge a soft roller with greasy ink and pass it over the surface of a plate prepared in such manner indicated (a specimen of which we have here), of course, where the light has acted there the greasy ink will take, because there is no water present. Where the light has only partially acted, the greasy ink will take partially; and where it has not acted at all, it will not take at all. The plate, after being immersed in water previous to printing, is placed in an ordinary printing press, pressure is brought to bear, and we get a print as the result. We will place the plate in this press. In practice we use

a larger press than this, which is hardly powerful enough to produce firm prints, but I will pass round some prints of the same subject taken before the lecture from the same plate. The paper generally used is enamelled paper, but, personally, I prefer ordinary drawing-paper. On the walls of the Exhibition you will see prints taken on ordinary drawing-paper, which I believe to be very effective, and some think more artistic than those taken on the enamelled.

In practice it is found that a stiff ink adheres well to the deep shadows, but not very well to what we call the half tones, so that a thinner ink is used for giving it a second coating; or you may use three inks, and give it a third coating, each one being thinner than the other. Therefore, you see we have great power of altering the tone or giving a different tint. Mr. Edwards, who was the patentee of this process, printed what I may call chromo-photographs by this method. He used parts of a negative, blocked out parts, printed a picture on enamelled paper; then he used another part of the same negative to print another section, and so on, piling up one colour above another in exactly the same way as a chromo-lithographer does. A few specimens were produced, but this modification does not seem at present to be very much worked.

I have been obliged to cut short a great deal of the description which I should have liked to have given in greater detail, but as time is limited I was obliged to do the best I could. In conclusion let me say that you will find specimens on the walls of all the processes I have spoken about, many of which are well worthy your attention.

Short as has been the description of the printing processes, it has, however, I hope, enabled you to see the giant steps which have been made in this branch of photography during the last quarter of a century. When we consider that carbon printing, Woodbury-type, and the other photo-mechanical processes belong to the last dozen years, it will be seen that enough has been accomplished to mark them as years of great progress. Every improvement has been the result of patient toil, generally of more than one person. Perhaps there is no field so open for

investigation as this branch of science, as I hope I may call it. There are many workers whose heads are always teeming with ideas, some of them very crude, perhaps, and impracticable, whilst others are well worthy of consideration, and have often led to some great advance in photography. I cannot conclude without asking you to accord your thanks to those gentlemen who have so kindly aided me in demonstrating the Woodbury-type and the other processes, for I am sure to them belongs any merit there may be about the work I have shown you.

The CHAIRMAN: Ladies and gentlemen,—I believe I shall only anticipate your wishes in proposing a very hearty vote of thanks to Captain Abney for his very clear and interesting lecture. In listening to him we cannot fail to be struck with the extraordinary ingenuity and the great fertility of resource which has been brought to bear on this intricate and important subject. Nothing short of the combination of many minds exercised over many years could have brought the art or science, for it is both an art and a science, to its present state of perfection. Looking at the subject from a scientific point of view, as in this place we can very fairly do, we cannot attach too high a value to the labours of those who have interested themselves in this subject. When to an accurate verbal description a scientfic observer can add a pictorial representation, no less accurate, of what he has seen, he has contributed the best possible aid to future workers in science. Nothing is better calculated to assist the reader of accounts of other experimenters than a faithful representation of what has been actually seen, and nothing is more calculated to mislead than an inaccurate one. Therefore the science of photographic representation of experiments and observations is of the highest possible value; and perhaps in no branch of science has this method proved of more importance or is likely to prove still more important than in that of astronomy. I mention this particularly, because, although Captain Abney in his modesty has alluded in only a very few passing words, to his own operations, the field of photography is one in which he himself has been very active. If we may turn for a moment from the scientific to the more practical aspect of the question, I myself, in common with

many others interested in printing, look with the greatest possible satisfaction on the success which has attended efforts for the production of photographic printing. When we shall ultimately be able to use blocks, engraved and prepared by photography, in the same manner as types, we shall have carried the art of printing illustrations, so far as we can at present see, to its very last stage. I will not, however, detain you with superfluous comments, but will conclude by asking you to join in a very hearty vote of thanks to our Lecturer.

THE ACTION OF ELECTRIC CURRENTS ON EACH OTHER.

LECTURE BY DR. SCHUSTER.

June 26th, 1876.

PROFESSOR CAREY FOSTER IN THE CHAIR.

THE CHAIRMAN: Ladies and Gentlemen,—I have been asked to take the chair this evening, and I believe my chief duty is the very simple one of asking Dr. Schuster to give us his lecture on "The Action of Electric Currents on each other." I may be allowed to say that it is a subject the experimental illustration of which presents considerable difficulty, at any rate when it is attempted to make the action visible to a large number; and therefore, although I have no doubt that such indulgence on your part is quite unnecessary to Dr. Schuster, still I must ask you to bear in mind that the effects which, I have no doubt, you will be easily able to see, can only be made visible with considerable difficulty and delicacy of arrangement.

DR. SCHUSTER: Ladies and Gentlemen,—The great interest which the public is beginning to take in scientific subjects has, no doubt, been caused by the many applications which this century has given to discoveries which have been made in the laboratory of the physicist and chemist. Amongst these applications the electric telegraph stands prominently forward, and it seems to us now incredible, that when this century began, the voltaic pile, by means of which we are enabled to send an electric current across the Atlantic, had not been invented. Galvani's celebrated experiment had only been made in the last few years of last century. His expériment was this: He connected two dissimilar

metals together, and touched with their two other ends the nerve of a frog. The muscle of the animal he then found to contract. It was reserved to Volta to point out what precise relations the electric current, as it was found afterwards to be, had with frictional electricity. In the year 1800, Volta constructed a pile by means of which he was first able to get a permanent electrical current. This discovery created a great interest all over Europe. It was at once thought that we should be able to arrive at the connection between electricity and magnetism which had been long suspected, and a good many people set to work to find out that connection. But, strange to say, twenty years elapsed without any results. Everybody in fact was trying to find out the connection where it was not,—and where it was, they did not look for it. At the end of these twenty years the interest which was taken in Volta's discovery had nearly died away, and it was believed to be barren where it was hoped to be fruitful.

One of those who worked hardest to discover a relation between electricity and magnetism was Oersted. He was not discouraged, and in the year 1820 he found out that the electric current affected the position of the magnetic needle. Oersted's discovery spread rapidly all over Europe. As soon as the news reached Paris, Ampère set to work; and as soon as the news reached London, Faraday set to work; and now followed twenty years of work and discovery with results such as the scientific world had not seen before.

I have the pleasure to-night to show you some of the instruments which have been used during those twenty years—instruments by means of which the original experiments were made, and on which it might be said that the whole science of electricity is founded. No two men could be more unlike each other in the way they published their experiments than Faraday and Ampère. Faraday always gives us the whole history of his thoughts; and if we read his works, we are led by degrees through ideas at first incomplete and crude, but always checked by experiment, until at last they become clear and sustained by a mass of experimental evidence. Not so Ampère; his ideas seem to

be clear from the beginning. He merely gives us the experimental evidence which is absolutely necessary for the proof of his theories, and we are unable entirely to follow the means by which he arrived at his theories. (See Maxwell, Electricity, vol. ii. p. 162.) Two months after the news of Oersted's discovery had reached Paris, Ampère published his first theory, and when, many years after, he had completed his work, after having surmounted mathematical difficulties which at first seemed insurmountable, he had not to retract a single word of what he had said in the beginning.

In order to explain to you, however, what these theories of Ampère and the results were, I shall have to say a few words about the state of science of electricity as it stood before his time. It was known a long time ago that if you rub two bodies together, those bodies have certain properties. They were said to be electrified. As the two bodies which are rubbed together have dissimilar and in many respects opposite properties, one was said to be positively electrified and the other was said to be negatively electrified. It was found that when a certain quantity of what was called positive electricity was added to an equal quantity of what was called negative electricity, the result was that the body appeared to be unelectrified, just as a positive quantity added to an equal negative quantity makes nothing. This is the origin of the terms positive and negative electricity. Now bodies, if electrified, behave in a different way according to their nature. If, for instance, I electrify one end of a piece of glass, that end remains electrified, and the other end of the glass does not show any signs of electrification; the electricity sticks, as it were, to that part of the glass where it has been generated, and does not travel along the glass. If, on the other hand, I electrify one end of a piece of metal, the other end is seen to be electrified as soon as I have electrified the one end. The electricity therefore spreads rapidly over the metal, and arrives at the other end of the metal as soon almost as it is given to the first end. It is said therefore that some bodies, like glass or ebonite, are non-conductors of electricity, and that other bodies, like metals, are conductors. For the present we have only to do with metals which

conduct electricity well. If therefore I electrify a piece of metal, the other end, as I have said, is seen at once to be electrified; but my *body* too is a conductor, and the earth is a conductor, so that if I hold the metal in my hand and electrify one end, the next moment the metal will appear to be not electrified, because the electricity at once spreads through my body and into the earth; and the earth is so large compared to this piece of metal that the amount of electricity remaining in the metal, vanishes in comparison with the quantity of electricity which has escaped to the earth.

If I want therefore to charge a piece of metal with electricity. I must not hold it in my hand, but must attach it to a non-conductor. One of these pieces is charged with positive electricity, and the other is charged with an equal quantity of negative electricity. If I join them together, what will happen? The positive electricity of the one will at once spread over the whole surface of the two together, and the negative electricity of the other will do the same; and as there was an equal quantity of positive electricity in one and of negative electricity in the other, the whole body will appear to be unelectrified. But now fancy that as soon as the quantity of positive electricity has passed, or while it is passing from one piece of metal to the other, and while the negative electricity is also passing in the opposite direction, a new supply of positive electricity is given to the one and a new supply of negative electricity is given to the other. Then this new supply will go on passing from one to the other; and if I continuously give a supply of positive electricity to the one end and of negative to the other, I shall get not a body charged with electricity, but a continuous current of electricity through that body—a continuous current of positive electricity from the end which I charge positively to the end which I charge negatively, and a continuous current of negative electricity from the end which I charge with negative electricity to the end which I have charged with positive electricity. We have then what is called an electric current, that is to say, electricity in motion from one part of a piece of metal to another part. Whenever we have an electric current, we have a quantity of positive electricity travel-

ling one way with an equal quantity of negative electricity travelling the other way. It was the invention of Volta, to construct an apparatus which should give him a continuous supply of positive electricity at one end, and of negative electricity at the other, so that a permanent electric current was established.

I have here what is called an electric pile; I cannot explain its working now, but it will suffice to say that one end of it is continuously kept supplied with positive electricity and the other end with negative electricity; and if I join the two ends together, a current of electricity will set up in this wire—a current of positive electricity from the end which is now to my right to the end which is to my left, and a current of negative electricity in the opposite direction.

This was Volta's discovery; and we now come to Oersted's discovery, which was made 20 years afterwards, in the year 1820. I have here a magnet, with a piece of red paper attached to one end, and a piece of blue paper to the other end. This magnet, as you know, if it is left free to swing in a horizontal plane as it is now, will set north and south; the blue end of this magnet will point to the north and the red end to the south. Oersted discovered that when he brought a current of electricity travelling along the magnetic meridian near the magnet, this magnetic needle will not point any longer north and south, but the current will tend to set it east and west. You will see that as soon as I bring this electrical current near the magnet, the magnet will change its position. There is a continuous current of electricity now passing through this wire, and you see that the magnetic needle changes its position as soon as I bring the current near it. The magnet, in fact, has set at right angles to the electrical current which I have placed above it, and the red end is now turned towards you. If I change the position of the wire with respect to the needle, you will see that the position of the magnet will change. I now bring the current underneath the magnet instead of above it, and now the blue end points towards you. But if now, without altering the position of the wire, I merely change the poles and send the current in the other direction, then again you will see that the needle will point in

the opposite way. The needle, therefore, tends to set at right angles to the electric current. If I have an electric current going along the table, the needle will tend to set from me towards you; and it will set in a different direction according as the current goes from right to left, or from left to right, and it will also tend to set in a different direction according as the current goes above the needle, or below it. Oersted gave a law by means of which we can at once find out what way the magnetic needle will set. This law says that if we imagine ourselves to be swimming in the electric current, so that the current enters our head and goes out through our feet, then on looking towards the magnet, the north end of the magnet will be deflected towards the right. If I did not see this battery here, I am not able to tell the direction of the current; but if I bring it above or below this magnetic needle, I can find out which way the positive electricity runs and which way the negative, by means of the law I have just given you. This was Oersted's discovery.

When Oersted's discovery first became known, two theories were set up. As the electric current acts on a magnet, and as it is known one magnet acts on another, some said the electric current behaves like a magnet, and electricity therefore must be reduced to magnetism, we must be able to explain an electric current by magnetism. But others said: Not so; just as well as you can account for electricity by magnetism, we shall be able to account for magnetism by electricity, and this was the theory which Ampère took up and which was the successful one. He tried to account for magnetism by electric currents; and he said at once: If I try to account for magnetism by electric currents, if in this magnet there are electric currents, which account for its magnetism, and if this wire acts on this magnet, then surely one electric current must act upon another. And he tried this and found it to be true, and discovered that one electric current acts upon another. He found that in order to get a mechanical action. we need not have a magnet and an electric current, but if we have two electric currents free to move, there will be an attractive or repulsive action. Here is the instrument which Ampère used, and by means of which he showed not only the attraction

of electric currents on each other, but also by means of which he afterwards went further and completely explained the action of magnets by electric currents

Here is another apparatus by means of which Ampère made various experiments. This coil must have been one of the first by which he has shown how we can account for the action of magnets by electricity, for he found that when he had a coil like this, and he sent an electric current through it, the coil will act exactly as a magnet, which fills up its interior, would act on any point a little removed from its ends.

I shall now try to show you some of Ampère's experiments. I have here a thick wire which is able to move easily, and I shall be able to show you, I think, that if I send an electric current through this wire, its position will be affected by another current which is placed near it. I send now an electric current through this wire, so that the positive current of electricity will run down the wire. If I bring this other wire through which the same electric current runs, near it, you will see that it affects the position of the movable wire; and if I reverse the direction of one of the currents, the wire will move towards the other side. These actions are not so strong as the magnetic actions, and, therefore, you are not able to see them so well, but at any rate those who are sitting near will see that one current has a mechanical action upon another.

You will be able to see a stronger effect if I send an electric current through this coil of wires. You will find that the coil will behave exactly like a magnet; if I bring this magnet near it, you will find that it affects the position of the coil just as one magnet would affect another. You know that if I have two magnets, and bring the two poles which point north together, they repel each other; and if I bring one pole which points north and another which points south together, they attract each other. Both north poles of these magnets are marked with red paper, if, I bring this end which is marked with red paper, near the end of the other magnet which is marked with blue paper, I shall have a strong attraction, and I can make the magnet turn entirely round by its means. On the other hand, if I bring the pole marked blue to-

wards the one which is marked blue, you will see a great repulsion, and I can drive it round in this way. This magnet you will see has the same effect on this coil, through which an electric current passes, as it had on a magnet. I can draw it round, or drive it round, according as I use one pole or the other. This solenoid, as it is called, or spiral, behaves exactly as a magnet would behave.

I shall be able to show you the same fact by means of this spiral here which floats on the water. I have a little electric pile affixed to it, so that one end of the spiral is constantly supplied with positive electricity and the other end with negative electricity. You will see the spiral will behave exactly like a magnet does. In fact, if the attraction is strong enough, you will see that the blue end will point towards the north, but as soon as I bring the blue end of the magnet near it, it will be driven away, whilst I can attract it with the red end of the magnet.

If I know the direction in which the electric current passes through the spiral, I can tell which end will behave like the north end of a magnet and which like the south. We have for this a similar rule to that which Oersted gave for the deflection of a magnet by an electric current; in fact, it can be deduced from Oersted's rule. If you look at a spiral with one of its ends turned towards you, and if a current goes round the spiral in a direction opposite to that of the hands of a watch, then the spiral behaves like a magnet, the north end of which is turned towards you.

These are the facts on which Ampère founded his theory of magnetism. He said: In order to account for the magnetic properties of iron, we must assume that each molecule of iron is magnetic. I go one step further, and assume that round each molecule of iron an electric current passes.

In the model which I have here, you will see these molecules roughly represented. They are all pointing the same way, and each is surrounded by an electric current indicated by the arrow heads. You will see that in the interior of the body, whenever I have a current going in one direction, I have close to it, so as to counterbalance its effect, a current going in the other direction. The only effect on a point outside the magnet must be caused by the electric currents going round the surface of the magnet; and

they, as you see, form one current going round the magnet, as the currents go round a spiral.

In order to prove this theory it was not sufficient to say in a general way a spiral will behave exactly like a magnet does; it will be repelled as a magnet would be repelled. Ampère had to measure the repulsion, and to show that it would be repelled in exactly the same degree, as a magnet would; and he not only wanted to prove this, but something more. He wanted to find out the elementary law with which each element of a current, each bit of this wire attracts each bit of another wire through which an electric current goes, no matter how this other wire is situated. This task was not at all a very easy one. We can very well find out the effect of one electric current on another, but we cannot experiment on a little bit of one electric current and find out its action on a little bit of another current. We have no bits of electric currents; we have always whole electric currents running in a closed circuit. Ampère could not therefore by experiment decide how a bit of an electric current would affect a bit of another electric current. The way he had to adopt was this. He had to assume all the possible ways in which a bit of an electric current could act on a bit of another current, then he had to see that these actions when added together would correctly represent the attraction or repulsion of the whole current, as found by experiment. It is a comparatively easy matter to measure the force which acts on a body, if we can set that body into oscillations under the action of the given force, but if we cannot do so, the task becomes a very difficult one. Various reasons prevented Ampère from using the method of vibrations, though this has been done later by Weber. Ampère got round the difficulty in a very ingenious way. He found out by experiment several cases in which a current under the action of other currents, though free to move, does not do so, because the action of the various currents counterbalanced each other. These cases of equilibrium were sufficient to determine the problem as far as complete circuits are concerned, and from them Ampère deduced what is called Ampère's law of the action of electric current on each other. I ought to say that the problem to find out the action of

each element of an electric current on another is not at all solved as yet. On the contrary, it is possible that for a long time to come we shall not be able to solve it, because we can imagine a great many different ways in which bits of currents act on each other, though if we take all these bits together and take the whole, we shall always arrive at the same law for the whole circuit; and if different elementary laws give the same law for the whole circuit, all these different elementary laws may be true. In order to get rid of this ambiguity, Ampère made a very fair assumption which would seem obvious to all of us, namely, that attraction and repulsion is always in the direction of the line which joins the two elements. The laws of magnetic attractions and repulsions and other attractions and repulsions are in that way; the two poles attract or repel each other in the direction of the line which joins them. He, therefore, made this assumption, and by means of this assumption he arrived at a certain definite law; but if we drop this assumption, and assume that the two elements may attract each other in any way whatever, then the problem is not at all a determinate one, and we can find out any number of laws. I must leave that point now and go to another part of my subject.

While Ampère was making these experiments, Arago had made a very interesting experiment indeed, and this experiment forms a transition from Ampère's work to Faraday's. I will just show it to you here. He had a plate of copper which he could rapidly revolve. If I turn this wheel, this copper plate revolves rapidly. I will put a plate of glass over the plate of copper, so that I can revolve the copper plate without touching the glass, which will remain perfectly stationary. I now place a magnet which is free to move over the glass plate, when the magnet will point north and south. The copper apparently does not at all affect the position of the magnet. If now, however, I turn the copper plate round, a very curious thing takes place. As soon as I turn it round, the magnet, which does not stand on the copper, but above the glass, entirely disconnected from the copper, turns round in the same direction; and if I reverse the motion of the copper plate you will see that

the magnet will first come to rest, and then turn round again in the same direction as the copper plate. Now what does this show? When the copper revolved, the magnet revolved with it; this shows that there must be some action between the copper and the magnet when the copper revolved, but not when it was stationary. Now, when I changed the direction of the motion of the copper, the direction, in which the magnet revolved, changed too. How must the action be, in order to account for the facts? The action clearly must be this: Each part of the copper disc which approaches the magnet must tend to drive the magnet away from it, and each part of the copper disc which went away from the magnet must tend to attract the magnet towards it. If each part of the copper disc which moves towards the magnet tends to push the magnet forward, the magnet will move from the parts which were coming towards it, and, as the other parts will attract it, the magnet will tend to turn round with the copper. Now we have a law which says that whenever a force tends to move a body in a certain direction, a force of equal intensity tends to move the body which is the cause of the original force in the opposite direction; if therefore each part of the copper disc, which moves away from the magnet, tends to draw the magnet with it, then the magnet in its turn must tend to attract each part of the copper plate which is moving away from it, that is to say, it tends to stop the motion of the copper plate. On the other hand the parts of the copper plate which move towards the magnet, tend to repel the magnet, and, therefore, must be repelled again by the magnet, and this shows again that there must be a stress opposing the rotation of the copper plate. I shall try to make an experiment here which will show you this a little more clearly. I have here a copper disc, which revolves near a magnet. I turn the copper disc round, and as soon as I pass the current round the magnet, as soon as it becomes a magnet, in fact, I shall have to use much greater strength to turn the plate round than I did before there was a magnet near it.

You see that what I have said is correct, that the parts of the copper plate which move towards the magnet repel the magnet

and are repelled from it, and that the parts which move away from the magnet are attracted by the magnet. What does this mean? I do not know of any other mechanical action of a magnet than that of another magnet or an electric current. The magnetic properties of a copper disc do not account for its behaviour. I must, therefore, have electric currents passing through the moving copper disc, and these electric currents must be always such that the action of the magnet on them tends to stop the movement of the copper disc.

It is now very easy for us to reason backward, and to see how from Arago's experiment we might have discovered the induction of electric currents. But at the time I am speaking of, Arago's experiment was interpreted in a different way, until Faraday by his immortal experiments completed the series of discoveries which Oersted had begun.

My time is up, and I cannot go into the details of Faraday's experiments. I will say this much, however. Faraday found that while a wire approaches or recedes from an electric current, an electric current passes through that wire, and the direction of the electric current is always such that the action of the other current on it tends to stop the motion of the wire. If we look at a magnet like Ampère as made up of electric currents, Arago's experiment is now easily explained. Professor Tyndall will no doubt explain to you next Saturday much better than I can do this inductive action. I must now conclude, apologising to you for the very imperfect way in which I have brought the subject before you.

The CHAIRMAN: Ladies and gentlemen,—The attention which you have given to Dr. Schuster's lecture shows that I shall be only anticipating your own feelings by asking you to record in a formal way your thanks to him for his lecture. For my own part I have listened to it with the very greatest interest, and have admired very much indeed the lucid way in which he has traced out the history of some of the greatest scientific discoveries that have been made at any time, and has shown that these did not happen accidentally or independently of one another, but that each contribution that is made to our knowledge of natural

phenomena prepares the way for further advances, and is itself rendered possible by those which have preceded it. Dr. Schuster has shown us very beautifully this organic connexion—one discovery growing out of another—among the subjects which he has brought before us to-night. I will ask those of you who share my feeling of gratitude to Dr. Schuster to express it in the usual way.

FARADAY'S APPARATUS.

LECTURE BY PROFESSOR TYNDALL, F.R.S.

July 1st, 1876.

SIR FRANCIS R. SANDFORD, C.B., IN THE CHAIR.

THE CHAIRMAN: Ladies and Gentlemen,—There is no special need for any remarks on my part in introducing to you Professor Tyndall, who has kindly volunteered to give us the lecture this evening.

Professor TYNDALL: Some time ago my friend Major Festing did me the honour of asking me to address you on this occasion with reference to the instruments of Faraday; and although at this season of the year rest instead of work is most suitable for me, I could not, considering my relations with that extraordinary man for sixteen or seventeen years of my life, decline the invitation of Major Festing, and therefore it is that I am here before you to-night to talk to you, in an entirely extempore manner, about the tools with which Faraday worked, and to make known to you, as far as the brief time at our disposal will allow, the use that he made of them.

In the year 1800 Volta announced his great discovery of the voltaic pile, so called because it consisted of a column of different metals—zinc and copper, we will say—arranged in pairs, with a wet cloth or wet cardboard between them. It was found subsequently that this arrangement of a vertical pile was inconvenient, and that it would be an improvement to have the

column turned horizontally, employing instead of the wet cloth or cardboard, a proper exciting liquid between the plates. Such a pile or, as it is sometimes called, a voltaic battery, is here before you. When we connect the two ends of this battery, by a wire, through the connecting wire passes what, for the want of a better name, we call a voltaic or electric current. In the same year in which Volta announced his discovery of the pile—and in fact before the full description of it, in a letter addressed by Volta to Sir Joseph Banks, the then President of the Royal Society, reached this country— it was found by Nicholson and Carlisle that when instead of making the wire continuous you cut it across, and dip the two severed ends of it in water, the water is decomposed by the voltaic current. From that hour attention was for a long time directed to these decompositions. With them the name of Davy is immortally associated; for it was by means of the current that he liberated from the alkaline earths those wonderful metals which we now know under the names of potassium and sodium.

A new impulse was given to enquiry through the discovery in 1820 by Oersted of Copenhagen, that when a wire carrying a current is brought over a magnetic needle, the needle is deflected from its meridian. This discovery excited an enormous amount of attention; as I have expressed it elsewhere, it precipitated upon itself the scientific thought of Europe. There was and is at Geneva a band of cultivated men who have always been quick in working out new scientific problems and in repeating any new scientific experiment. They got intelligence of Oersted's discovery, and in September they repeated the experiment which he had first made in July. A member of the French Academy of Sciences, happening to be in Geneva at the time, saw the experiment, and on the 11th of September he described to the Academy in Paris what he had seen. One of the greatest scientific intellects that ever lived, the celebrated Ampère, was present at that meeting. He listened to the account of Oersted's discovery, went home, and precisely a week afterwards, that is to say, on the 18th of September, he brought before the Academy additional facts and phenomena,

which in number and importance, considering the brief time devoted to their discovery, have never perhaps been equalled in the history of science. He discovered not only that the current acted upon a magnetic needle, but that currents acted upon each other; he devised apparatus by which currents were rendered moveable and by which moveable currents were acted upon by fixed currents. In this single week he developed the laws of what we now call electro-dynamics.

Ampère's experiments led him to associate in the most intimate manner voltaic electricity and magnetism. They finally led him to enunciate the bold and beautiful theory that an ordinary magnet was nothing more than an assemblage of electric currents circulating round the atoms of the steel, and he proved that by properly suspending a spiral through which an electric current was sent the spiral would show the effects of magnetism. Such a delicately suspended spiral is here before you. With each end of the copper spiral is connected a smaller spiral of platinum wire, through which the current circulating through the larger spiral will pass. The current will ignite the platinum wire, so that at the two ends of this suspended spiral of copper wire you will have two little incandescent platinum lamps announcing the passage of the current. The experiment requires very delicate adjustment, but still you notice that the spiral acts like a magnetic needle, and sets itself in the magnetic meridian, in obedience to the solicitation of the earth's magnetism. When moreover, the end of a steel magnet is brought near one end of the spiral, you observe that repulsion ensues. If the other end of the magnet be presented to the spiral, attraction ensues; so that you can in spirals of this kind develope the phenomenon of polarity and all the other phenomena of magnetism. On the 18th of September, 1820, Ampère made known these beautiful experiments to the Academy of Sciences, and a week subsequently, having been working in concert with his colleague Arago, he showed that the wire through which the current passed differed from an ordinary wire in a manner now to be made plain to you. Through this wire the current will pass immediately, but before it

passes I will place the wire in iron filings. The wire has not the slightest attraction for these filings; but, as soon as the circuit is established, on lifting the wire out of the iron filings they adhere to it, making it almost as thick as a quill. They are held there solely by this power, to which we give the name of the voltaic current. The moment the current is interrupted down fall the filings, because the attraction has ceased. This discovery was brought before the Academy of Sciences on the 25th of September, 1820.

Multitudes of facts were soon added by Ampère and others to those already discovered. Faraday struck into this field in the year 1821. He wished to know all that had been done in it, but he found the literature so scattered, that there was great difficulty in obtaining a connected view of what had been accomplished up to that time. In order to instruct his own mind he set himself to write a history of electric magnetism.

This essay was published anonymously in Thompson's 'Annals of Philosophy,' and here was the portal by which Faraday entered on his great career as regards electricity. He worked at the subject with very considerable results. It was then thought by various able men that interactions, not yet known, might exist between voltaic currents and between such currents and a magnet. Faraday sought and discovered such interactions, and I have here one of the instruments devised by himself, intended to show the rotation of a current round a magnet. Through the centre of a little vessel containing mercury passes a little magnet, and dipping into the mercury is a wire loosely hung at its upper end. With the most simple devices Faraday accomplished his experimental ends, and this is one of them. The apparatus is so small that even those close at hand could hardly see it; and we will, therefore, throw the shadow of it upon a screen. You observe that the wire rotates the moment a current is passed through it. This was Faraday's first discovery in the domain of electro-magnetism. He subsequently devised a larger apparatus, which my assistant, Mr. Cottrell, will now place before us. The ingenuity manifested by Ampère in rendering his currents moveable, so that they could

be freely acted upon by other currents, or by magnets, was extraordinary. Here is an example of the devices that were then applied. A moveable arm is supported by a point which offers exceedingly little friction and dips into a capsule containing mercury. A large bar magnet, round which the movable arm can rotate, stands erect. Making the circuit, we send a current through the moveable arm, and that current acted upon by the magnet rotates completely round the magnetic pole. The direction of the current, moreover, governs the direction of the rotation; because by reversing the current in that moveable wire, we at once stop this motion and produce rotation in the opposite direction. This then, as I have said, was Faraday's first experiment on entering the domain of electro-magnetism.

He was subsequently engaged upon the liquefaction of gases; and here he established the great truth that what we call gases are merely the vapours of liquids with a very low boiling-point. He liquefied chlorine in 1823. Turning his attention to chemistry, in 1825 he discovered the substance called benzole—the basis from which all those aniline dyes which are now so frequent, have been developed. Soon afterwards he devised that beautiful instrument which you are, no doubt, all acquainted with, the chromatrope. He then went on to investigate the vibrations of plates, and solved some of the difficulties that had beset the subject previously. This brought him to the year 1831, when he was 40 years old, having been born on the 22nd September, 1791. At this time the thought occurred to him that an electric current in passing through one wire might exert some action upon another wire placed near the one through which the current passed. His apparatus is here—his own series of wires which are placed in the collection of scientific instruments. These old coils are for ever memorable, for they were made by Faraday's own hand. But for the sake of plainness I will use a larger coil. Here are two wires wound side by side round a reel, and entirely insulated from each other. One of these wires is connected with an instrument called a galvanometer, in which the discovery of Oersted is turned to account. It is the peculiarity of our science, that no sooner

is a discovery made than it is converted into the germ or root of further discoveries, so that, instead of being an aggregate of facts, science propagates itself like a living organism. The galvanometer consists of a wire coiled round a magnetic needle, and I will make with it exactly the same experiment before you as Faraday made in 1831. I cannot tell you what his reason was for making it. Indeed, men like Faraday have an instinct in those matters beyond what they themselves are always able to explain. From a battery of five cells I send a current through one of the wires of the reel referred to a moment ago. The other wire, which has no connection at all with the battery, is united with the galvanometer, and if you look at its needle you will observe what Faraday observed — a slight motion of the magnetic needle the moment I establish the circuit. The cause that produced that motion vanished immediately; and as long as the current continues to flow round the reel, there is no action whatever upon the needle, which returns exactly to zero, as it was before. During the continuous flow of the current through the one wire, it has no sensible effect on the other; but the moment I intercept the current you observe a deflection of the needle on the other side.

This was Faraday's first observation with regard to these currents, to which he gave the name of *induced currents*. He found that when he made his circuit, and at the moment of making it only, a current was set up in the wire unconnected with the battery. He found also that on breaking the battery circuit another momentary current was set up, which deflected the needle in the opposite direction. This was to him profoundly significant. He went on varying and exalting the action of these induced currents. He was perfectly convinced, from the experiments of Oersted, and from those of Ampère, of the intimate alliance between electricity and magnetism, and this caused him to make the following experiment. Into a coil of wire, like that before you, he introduced a magnet, and saw instantly that a distant galvanometer needle moved in obedience to the influence. But as long as the magnet remained motionless within the coil there was no action whatever upon the needle. It was only a momentary

action produced during the time that the magnet was being forced into the coil. I repeat Faraday's experiment, and obtain his result. And now I simply withdraw the magnet, and you observe a prompt deflection on the other side. Faraday next took a core of iron, placed it within a helix of copper wire, and magnetised the core slightly by bringing a magnet into contact with it. He was unable to alter the magnetic condition of the space within the helix without producing in the helix one of those induced currents. I bring, as Faraday did, a magnet up against the core, and you see the deflection of the needle. It moves for a certain distance, but it is only during the moment in which magnetism is being set up in the core that this current is observed. I now withdraw the bar magnet from the core, the magnetism of the iron subsides, and this subsidence is accompanied by an induced current opposite in direction to the former one.

Another piece of apparatus that you will find in the collection is this iron ring.* Faraday had two insulated wires surrounding the two halves of this ring and separated from each other; one wire is intended to go to the battery, and the other to the galvanometer, and they are quite apart. I send a current through one of these coils. It magnetises the iron ring, and this evokes an induced current in the other coil, which declares itself on the galvanometer. There is an ample deflection which can be seen by all. It is only during the act of magnetization that this effect is produced. It is a perfectly momentary effect. I intercept the battery current, and the subsidence of the magnetism in the iron ring, as in the case just brought before you, causes a deflection of the needle in the opposite direction. We are now travelling in the footsteps of Faraday. We might vary this experiment by taking simply a coarse, loose coil of copper wire over-spun, like that before you, with cotton for the purpose of preventing its different parts from coming into contact with each other. I simply thrust this bar magnet through the coil, and you see the wonderful action produced on the needle of a galvanometer connected with the coil. I wish I were able to tell you, or that you were able to tell me,

* In Foley's magnificent statue of Faraday this ring is held in the hand.

what has here occurred between the magnet and the coil—what is the state of the space between them. We do not now know what it is, but assuredly we shall know by-and-by; it is a subject for future investigation. In withdrawing the magnet from the coil the needle swerves suddenly in the opposite direction. At every entrance of the magnet into this coil, and at every withdrawal of the magnet from the coil, you have these effects produced.

At the time that Faraday made these discoveries, an unsolved problem presented itself to scientific men. Observe this disc of copper associated with this whirling table. Over that disc is suspended a magnetic needle. The moment the disc is caused to rotate, the magnetic needle immediately follows it. You might be disposed to say that this is due to the air currents acting upon the magnetic needle, but that is not so. You may introduce between the disc and the magnetic needle a plate of glass, which entirely protects it from all air currents, and precisely the same effect follows. Turning the disc in the opposite direction, the needle stops and reverses its motion. This is a great discovery, for which we are indebted to Arago; but Arago did not solve it, nor did he pretend to solve it. The experiment was repeated and the subject was investigated by the most eminent men of the day, among others by Sir John Herschel and Mr. Babbage. They all had their notions regarding it, but not until Faraday made the discovery which I have shown you, was any light thrown upon the "magnetism of rotation" discovered by Arago. Faraday immediately saw that we cannot have a rotating copper disc in the presence of a magnetic needle without the production of induced currents in the disc, and we cannot have such induced currents without interaction taking place between them and the magnetic needle. He demonstrated the existence of these currents, and thus entirely solved the enigma which had previously perplexed scientific men.

Now, I wish you to be acquainted not only with Faraday's mode of experimenting, but with the imagery of Faraday's mind in dealing with these subjects; and for this purpose I will show you an effect familiar probably to most of you, and that

is, the manner in which iron filings arrange themselves round a magnet. In order to render it plainer we have here two very small magnets, a magnified image of which we can throw upon a screen. Over these magnets we scatter iron filings, and you observe the beautiful manner in which they arrange themselves. The arrangement of the filings in those lines, which were formerly called magnetic curves, but which Faraday called "lines of magnetic force," furnished him with a conception which was present in his mind throughout all his scientific life. Faraday found that, in order to produce his induced currents, he must cut, by the wire in which the induced current is to be evoked, the lines of magnetic force. This was also true of terrestrial magnetism. A dipping-needle in this room would form an angle of between 70 and 80 degrees with the horizon. Here is an apparatus, constructed for me by one whom we all lament both as a mechanician and as a man, the late Mr. Becker. I will cause this apparatus, which consists simply of wire coiled round a frame, to rotate so that the axis of rotation shall coincide with the line of dip. The two ends of the coil passing round the circular frame are connected with the galvanometer. When it thus rotates there is no action whatever upon the needle.

Faraday figured the earth as a magnet; he figured these lines of dip as lines of force, so that if you could scatter iron filings in the air of this room and could withdraw them from the action of gravity, causing them to float free from the attraction of the earth, they would arrange themselves in lines parallel to the axis of rotation of that coil. By placing the coil in another position I destroy the parallelism of its axis with the line of dip; it will now cut the earth's lines of magnetic force, and we get immediately, by the action of the earth's magnetism, a large deflection of our needle. I turn the coil from right to left; the deflection of the needle is in a certain direction; I turn it backwards and the needle moves in the opposite direction.

In the case of terrestrial magnetism, which is much feebler than that of our ordinary magnets, we shall operate with greater advantage by employing a more delicate galvanometer, and I

will use for this purpose that very beautiful instrument for which we are indebted to Sir William Thompson. In the one we have been using, we employ long needles such as were frequently used by Faraday himself, but in this reflecting galvanometer there is a very short needle protected from the action of air currents. Associated with the needle is a mirror, upon which if a beam of light be thrown, it is reflected back upon the screen. Every motion of the needle is accompanied by the motion of the spot of light upon the screen.

Here then is a single loop of wire; and here is a bar magnet. The instant I put the end of the magnet into the loop, the image reflected from the mirror travels over the screen. The induced current evoked in the single loop of copper wire subsides immediately, and the needle of the galvanometer comes to rest. I now simply withdraw the loop from the magnet; you notice a deflection of the spot of light in the opposite direction. The motion of the loop gives me the most complete command over the motions of the galvanometer.

Again, here is a flat spiral of covered copper wire through which is sent a current from our battery. Here is another similar coil which is connected with the galvanometer. When this second coil approaches the other, and as long as it continues to approach, you get an induced current evoked in the approaching coil; and when it retreats you get an induced current evoked in the opposite direction. These opposite induced currents are declared by the opposite movements of the spot of light on the screen.

You cannot move this second coil in the presence of the one through which the battery current flows, without producing in the former one of those wonderful induced currents. This is also true with regard to the action of terrestrial magnetism. Taking, as before, our coarse coil of covered wire, and connecting its two ends with the reflecting galvanometer, I simply lift that coil of wire, or one part of it, off the table; you observe what occurs. The coil cannot be turned in any way except one, that is, by making its axis of rotation coincident with the earth's lines of magnetic force, without producing a deflection of the

needle. If I lift it up in the slightest degree, the current evoked by the magnetism of our earth appears. If I let it fall, the deflection is in the other direction; so that, practically speaking, there is not a gentleman or lady in this assemblage who wears a ring, who can move his or her finger without evoking in the ring one of those wonderful induced currents. Here is Faraday's own apparatus by means of which he obtained these effects. It is a rectangle of copper wire, made possibly by his own hands. If we place that rectangle thus, so as to make the axis of rotation coincide with the line of dip, we get no induced current in the rectangle, but in all other positions of the rectangle Faraday obtained these currents.

For the purpose of gathering up these opposite currents and sending them all in a common direction, an instrument, called a commutator, was introduced. It is now a well-known instrument, and you will find many examples of it in the science collection.

Another beautiful experiment of Faraday's consists in obtaining induced currents by the rotation of a magnet round its own axis. Half the magnet is plunged in mercury. The top of the magnet is connected with a wire going to one end of the galvanometer wire, and the mercury is connected with the other end of that wire. By simply turning such a magnet round its own axis, Faraday obtained these induced currents. The thoroughness with which he exhausted these relations of magnetism and electricity is astonishing.

Faraday himself, when first investigating these small actions, said, "I am far less desirous of exalting these effects and of making them strong than of adding new facts and principles. I know full well that the time will come when these effects if necessary will be exalted." That prediction has proved true. If you go down to Dover and cross over to Calais at night, you will observe upon the South Foreland two lights of solar splendour shining over the sea. Those lights are produced by currents, the germs of which have been now placed before you, and which were discovered by Faraday in 1831.

I asked you particularly to bear in mind the direction of

Faraday's lines of force round the small horse-shoe magnet, and I now want to show you an experiment which illustrates in a manner more striking perhaps than any you have yet seen, the influence of those celebrated lines of force. Here is the magnet with which we operate at the Royal Institution, and we have it in our power to set it horizontally or vertically. This great horse-shoe magnet is exactly similar to the smaller one, the image of which was thrown on the screen. Between the two poles of this magnet is placed a cube of copper, which you will find in the collection. It is suspended from a thread which has a great amount of torsion, and a beam of light is thrown upon this little pyramid, composed of four bits of looking-glass. I will now liberate the cube; the beams of light rotate, reflected from the bits of looking-glass, and you see a band of light, due to the rotation of the cube. By exciting the magnet, that motion is instantly stopped, and the band becomes a spot. The torsion of the thread struggles to twist the cube round, but it moves as if immersed in honey or treacle or some other viscous medium. The reason is, that you cannot turn the cube between the poles of the magnet without exciting induced currents in the cube, and you cannot excite these induced currents without an interaction being set up immediately between them and the magnet. This is what stops the motion and causes the cube to turn as if it were moved through a viscous medium.

We will now so place the magnet as to cause the axis of rotation to coincide with the lines of force passing across from pole to pole. The cube occupies precisely the same position between the poles as before, but in this case its axis of rotation coincides with the lines of magnetic force, and you find, when we strike the proper line, that the making or breaking of contact has not the slightest influence upon the rotation of the cube. If, moreover, between the two poles of this magnet you move a plate of copper as a saw, you can hardly resist the impression that you are cutting cheese; and this is entirely due to the action set up between the magnet and the induced currents evoked in the copper by this motion. If you drop half-a-crown from a height between the poles of Lord Lindsay's great magnet, on coming between the poles, it is arrested,

and slowly and deliberately descends between them because of the currents set up by its motion across the lines of force.

We now pass on to another discovery, the full fruit of which must be reserved for the investigator of the future. It is what Faraday called the magnetisation of a ray of light. In 1827 he, in association with Sir John Herschel and others, established a furnace at the Royal Institution for the purpose of making glass suitable for optical purposes. They wanted a very powerfully refracting glass. It was produced, but unhappily it was very easily tarnished and never came into use as optical glass. With that heavy glass, however, Faraday made some of his most important discoveries, among others the one which I am now going to illustrate before you. Sturgeon showed that if you coil a wire round a piece of iron, and send a voltaic current through the coil, you confer upon the iron the most intense magnetism; and here we have a piece of a link of a chain-cable, and round about it we have a coil of wire which will enable us to render the link a powerful magnet. Upon the poles of the magnet are two perforated pieces of iron, and from one piece to the other a bar of Faraday's heavy glass is laid. We will send a beam of polarised light through a lens, through the perforations of the poles, through the heavy glass, and finally through a piece of Iceland spar, called a Nicol's prism. The Nicol is so placed that there is very little light. If I turn it a little round, the light comes through it; but I turn it so as to get the maximum darkness. At the present time the magnet is merely a piece of inert iron. I make the circuit, excite the magnet, and you immediately see the revival of the light on the screen. This is what Faraday called the magnetisation of a ray of light. When the circuit is broken, the light subsides.*

Time permits me to say only one word with regard to another great discovery of Faraday's. We have spoken of these induced

* The rotation of the plane of polarization was also shown during the lecture by means of a plate of right handed and left handed quartz. A semicircle was formed of each crystal. Bringing both semicircles to a common puce colour, on exciting the magnet the one became red and the other green. The reversal of the current caused the reversal of the colour.

currents, and of the magnetisation of light, and I have shown you the apparatus with which he operated. Now I wish to show you an experiment on an exceedingly minute scale, but one of very great theoretical importance. In reference to this experiment and to this discovery, the quality of Faraday's mind was conspicuously shown. The effect had been observed before him, but he knew nothing at all about the observations which had been made. It had been observed by Brugmanns and others that antimony and bismuth were repelled and not attracted by the magnet. Faraday re-discovered the fact of repulsion with his heavy glass to which I have referred. He then passed on to bismuth and to antimony and other substances. It was one feature of Faraday's mind, that he never rested content until he brought the strongest power he could evoke to bear on the substances he operated upon. He pushed things as far as the state of science then existing would allow him to push them. He brought powerful magnets to bear upon matter of all kinds, and he found that matter generally fell into two classes: into that class of bodies ordinarily called magnetic, which were attracted by the magnet, and into the class which he called dia-magnetic and which were repelled by the magnet. The most conspicuous instance of repulsion by the magnet is exhibited by the metal bismuth. We have here a little pellet of bismuth placed in front of a mass of iron associated with the magnet. On exciting the magnet the pellet is repelled, or driven away to some distance from the magnet. When contact is broken, it falls back again. This is the dia-magnetic repulsion, not largely exhibited, but sufficient to show you that repulsion is there.

I will refer to one other experiment because it reveals in some measure the nature of the speculations in which Faraday indulged in the later and more mature years of his life. To show you the influence of the medium surrounding a body on its magnetic deportment, a small bulb of glass, containing a comparatively weak solution of proto-sulphate of iron, is here suspended in a stronger solution of the same substance.

What Faraday wanted to bring out was this, that a body really

magnetic, if surrounded by a medium more magnetic than itself, will be repelled. This is the principle of Archimedes as applied to magnetism. Here then is the cell containing the solution of proto-sulphate of iron, which is a green liquid. The excited magnet will act upon this bulb, which in air or water would be attracted. You see the strong repulsion of the bulb, simply because it, a magnetic body, is immersed in a magnetic medium stronger than itself. As I have said, this is an experiment which lets you into the manner and mode of Faraday's speculations during the later years of his life. Philosophers before his time had looked at the action of the magnet upon filings, without dreaming of any substratum in which those iron filings were so to say immersed. According to such philosophers, this was an action at a distance, every particle of the iron filings arranging itself as a little magnet would do under the circumstances. Faraday first of all was exceedingly cautious of using these so-called lines of force otherwise than as a kind of symbol or conception, which helped him in unravelling those complex phenomena of magnetism; but as he grew older, he grew more and more to think that there was in space a medium acting on those iron filings, and that really those lines of force did not depend on the iron filings, but existed in space, irrespective of them. This experiment I have now shown you led him to consider that there might be a magnetic medium in space; that this medium might possess magnetic properties; and that substances which are apparently repelled by the magnet, might really not be repelled at all, but that they might be immersed in a medium more strongly magnetic than themselves, and be repelled as the bulb of proto-sulphate of iron was repelled in the foregoing experiment.

What I have brought before you to-night regarding Faraday's researches and discoveries, you must accept as the merest specimen of the work which this great man accomplished. I say that as time went on, you find him more and more occupied with these lines of magnetic force; this conception of a medium in space, similar to the medium concerned in the propagation of the waves of light, took more and more possession of

him; and the latest attempt he was engaged upon, when his powers began to fail, was the determination of the question, whether magnetism requires time to propagate itself from point to point of space. I have in my own possession beautiful little wheels and pinions, and little moveable mirrors which he intended to apply to the solution of this question; but before he could bring his wonderful powers to bear upon its solution, he ended his noble life at Hampton Court on the 25th August, 1867.

The CHAIRMAN: Ladies and Gentlemen,—I am sure I have your full sympathy when I tender on your behalf to Professor Tyndall our grateful thanks for the very interesting and instructive lecture that he has given us to-night; interesting to all of us, and most instructive to the students of the subject he has so ably handled. I must however express to Professor Tyndall, on the part of the Education Department, our regret that we have not yet been able to secure a somewhat larger hall, in which he could address a more numerous audience. It is not our fault; and in a few years I hope, when a competitive examination in science is made one of the conditions of admission to the controlling department of the State, we shall be able to give him a room, of which the foundations will be steadier, while at the same time it will accommodate a more numerous though not more appreciative audience.

It is one of the happy results of the collection of scientific apparatus, which has been so recently placed here, that we are brought face to face with the simple apparatus by which great men have made their wonderful discoveries, but it is a still happier result that we are brought face to face with the great and simple men who have the ability to explain, to illustrate, and carry on their predecessors' work. We have one of those men before us to-night, and in your name I beg to tender him our hearty thanks.

AIR AND AIRS, AS ILLUSTRATED BY THE MAGDEBURG HEMISPHERES AND BLACK'S AND CAVENDISH'S BALANCES.

By The Right Hon. Lyon Playfair, M.P.

July 3rd, 1876.

The Right Hon. Lord Aberdare in the Chair.

The Chairman: Ladies and Gentlemen,—On inquiring what were the duties expected from me this evening, I was told it was my business to introduce to your attention the Lecturer who is now to address you. It seems to me that to those who have paid any attention whatever to the extraordinary progress we have made in the last 30 years, the name of Dr. Lyon Playfair requires no introduction. During the whole of that time few men have been more prominently devoted than he has in the progress and improvement of his fellow-countrymen. Whether employed in making inquiries on the appointment of the government into matters affecting the public health or public morality, or engaged in the administration of those schools for the promotion of science which have done so much to add to the knowledge and intelligence of the working classes, or whether as a member of Parliament representing one of the most learned constituencies in this country and always advocating the progress of education, and applying his scientific knowledge to sanitary measures; in all these matters Dr. Lyon Playfair has played so prominent a part that it does seem to me that any introduction on my part of such a man to the audience is mere impertinence and altogether superfluous.

Dr. LYON PLAYFAIR: My Lord, Ladies, and Gentlemen,—To night, I intend trying to interest you in a chapter in the history of science. That chapter is related to the progress of our knowledge in regard to air. Now air is of all substances the most familiar to us, and the one which from the earliest time, when man's intelligence dawned out of a savage state, must have specially attracted his attention. But I think before the lecture is over, you will see that the untutored senses of man are perfectly insufficient to enable us to know any one subject; that is to say, the untutored senses of man without the experience and conceptions which accumulate around him as he progresses in history and in the knowledge of the world; because, if it were otherwise, surely such a substance as air must be the first thing with which he would become intimately acquainted. His life, his first breath, depend upon the air which surrounds him; the last act of his life is his inability to respire the air, for the inability of inspiration causes his death. Therefore in every phase of his life this familiar substance meets him. It fans him sometimes with gentle breezes, and occasionally batters him with storms, but never for a moment from his birth to his last breath does it cease to press upon his attention, and yet it has taken us the whole of the time since man came into the world to know as much about it as we do now, and yet our knowledge of it is still extremely imperfect. I want you to go very far back with me before we come to those special kinds of apparatus which form distinct epochs in the history of the discovery of air, and which I hope, rude as they are in appearance to you, will have a new value when we have understood how they became important. I propose to take you as far back as 640 B.C. At that time men began for the first time to study the peculiar subjects around them. The first subject that they studied was not air but water. There was an old Ionian philosopher, Thales of Miletus, who travelled very far for his time; he went into Egypt, and he saw there the very important relations of water to the fertility of the soil. He noted that the soil was sterile; that plants would not grow until the Nile came down in its large flood and irrigated the land; and then he found that the sterile earth became fertile, that plants and vegetation grew luxuri-

antly: and he, with his untutored senses, but still with a philosophic spirit trying to explain all things around him, began to think that everything in the world was made of water. It was clear that earth was made of water, because when the water went over the earth the sterile land became fertile. It was certain that plants were made of water, because the plants grew when the water came, so that the water was converted into the plants. It was obvious that the sun was made of water, for you could see it in the evening, tired with its course during the day, plunge beneath the western wave and come out in the morning mightily refreshed with its huge drink. Certainly proofs were abundant that the sun was made of water, because it sent down its scorching rays during the day, and licked up the water from pools and lakes and took them back again into the sun to refresh itself in its hot course. All these things were evidences of the senses, and so far as the untutored senses went, Thales was quite right to say that everything was made of water.

Some time after Thales gave the first impulse to philosophy by trying to give an explanation of the familiar things around him, another man thought he would make a considerable capital by making everything out of air, and it is to him that I come next. But it was the impulse that Thales first gave with regard to water that made Anaximines such a great philosopher in respect to air. Anaximines who came 548 years B.C. alleged that Thales was altogether wrong in alleging that everything is made of water; on the contrary, everything is made of air. Thales, according to him, made a prodigious mistake in thinking that water is so important, because water is produced by air, and you see this in the transparent air that surrounds the world; it condenses into clouds, and they into drops of water: but it is clear the air itself squeezes out that water when it chooses to be converted into it. It is air, then, which produces water; it is not water which is the primordial element, but air, and so he built up everything from air, just as Thales of Miletus had built up everything from water. The very world itself is composed of air, for you see it floating like a broad leaf upon it. Therefore all things in the world itself have been produced from air. Very little more was required by

Anaximines to be convinced that air was the essence of life. You require air to breathe; you could not live without it; if the air is taken away you die. It is quite clear therefore that the essence of life consists of air: nay, a little more, you see the soul is composed of air. According to Anaximines, the soul itself is finely rarefied air, and the conclusion, so far as he knew the matter, was inevitable, that all these things came from air, even fire, because nothing will burn without air. A little further, and God himself was made from the infinite air, and the gods and goddesses were different kinds of aerial emanations. It is easy to smile, for these things now appear so utterly preposterous, but they were the early dawnings of philosophy, and the first attempts of men to understand the things around them. Therefore we have to give them a respectful consideration.

More than a century after this, in the fifth century before Christ, appeared a great philosopher, Diogenes of Appollonia. He took up the old view of Anaximines which was nearly forgotten, but he discussed it more as a psychological than a physical problem; he connected it more with the soul and with sentiment than with the physical effects which Anaximines had given to air. He said Anaximines was perfectly right, that the soul was made of air, but he had not gone far enough, so Diogenes went a great deal further. He thought the whole world was made of air; that air was the soul of the world, the *anima mundi* which produced everything good that the world possessed in it, and was the essence of order in the universe. All the beautiful harmony which we now know as law, he conceived to be a sort of fetish intelligence existing in air; and air being an intelligent spirit in itself, having intelligence residing within it, like all intelligent beings, had varying humours. When it was in a good temper we had a balmy gentle breeze, when it was in a fitful temper we had sudden gusts of wind, and when it was in a towering rage we had thunder-storms. In his fetish worship, and in the infancy of science, it was a much easier thing to think every thing had a special primordial spirit within it than to know, as we do now, that everything is subject to infinitely wise and unvarying law.

Hence came the name of air, geist, gas, or ghost. Some of the spirits of air were evil; sometimes when it was in a revengeful spirit, men went down wells and were choked. Sometimes as in fiery mines, there were explosions, for then the air was in a wild state of tempest and rage, and so he went on with his explanations. Life of course came from air, and not only life, but the different kinds of soul. It was obvious that as man was erect with his head up in the air, he breathed the pure air; and the upper air gave him the intellectual soul of man, but the brute beasts who had their noses near the ground, breathed in the damp vapoury air, and they had brutal souls, and that was the difference between men's souls and brute souls.

Then came a great era, 348 B.C., when Aristotle began to apply his tremendous mind to science. Aristotle was an experimental philosopher. All the previous philosophers were not experimental philosophers in any way, they were disputacious philosophers; they disputed about all the things they saw before them, but they never thought of applying or testing by experiment the explanations which they gave. But Aristotle, being very friendly with his great pupil Alexander, had numbers of things sent to him from all parts of the world where Alexander was making war, and he began to be experimental, not so much physically, but he was a great anatomist, and his anatomical descriptions of animals and plants at that time are still very remarkable. He was the first chemist that began to be scientific, because he said all substances are composed of earth, air, fire, and water, and these elements for a long time served chemistry; in fact, it was not until 1774 that we found air was not an element. Up to that time, it was one of Aristotle's elements, a substance, *sui generis*, that could not be broken up into two or more sorts. Therefore Aristotle was the first chemist, for he induced men to think that if all substances were composed of earth, air, fire, and water, we must be able to get these elements out of bodies, and so men began to be chemists and tried to pull these elements out of various kinds of matter to see how much air, how much earth, how much fire, and how much water, were in them. Aristotle also knew that air was a material. He was

aware that if you turned a tumbler upside down, with air in it, water does not rise, and therefore air must keep out the water; and if air was matter, you must naturally conceive that air had weight. However, Aristotle does not seem clearly to have known that air had weight, although it followed as a necessity from it being matter.

Now I make a great jump from the time of Aristotle, and come to the year 1100, because nothing was done in the interval worth telling you of with regard to air. But about that year a great Saracen called Alhazen began to think of air, and he thought of it in a most extraordinary manner. We do not know anything about Alhazen's history, but we know something of his works, and they were quite of an astonishing nature. He knew as well as Torricelli and Galileo that air had weight. He was the first man that ever applied a balance to physical phenomena. Alhazen experimented, balance in hand, with air. He knew that the lower strata of the air are heavier and denser than the upper strata. He discovered the laws of refraction in air, and explained how twilight came from refraction of the rays of the sun, and so on. He gave a wonderful impulse to our knowledge, but what was the most astonishing of all was, that he did not believe that air was infinitely scattered throughout space. Former philosophers thought that air was infinitely spread throughout space. What could prevent it? We see no limit to air when we look up; and as there is repulsion between its particles, why should it not be present all through space? But Alhazen, through his discussion of the habits of air, said it could not, and he fixed its distance at fifty-eight miles and a half. We now know that it is about forty-eight miles, so that he made an immense step in our knowledge of air.

I pass from the time of Alhazen, because there is nothing worth telling you from him down to Galileo, in 1630, and here we begin to come to our modern philosophy of air. Galileo proceeded to investigate what was the action of a common pump. You know that when you suck the air out of a tube the water rises. Aristotle had said the reason is very clear, nature abhors a vacuum, and the water rushes up into a vacuum because nature

abhors it. But Galileo found that nature only abhorred a vacuum up to the extent of about 33 feet, that the water in the pump would rise up as far as that, but no higher. Why was that? You could suck out the air beyond 33 feet, but the water would not rise, and Galileo began to discuss this question why it would not. If it is because nature abhors a vacuum, is nature reconciled to a vacuum at 33 feet? Galileo saw that it was obviously the weight of the air pressing on the surface of the water which made it follow the air when it was pumped out, and that that was the reason the weight of the air could support a column of water 33 feet high, but could not support a column 36 feet high. This explanation became a splendid point in the history of air, for it was evident that air must have weight, and that you could estimate its weight.

Then came Torricelli. My friend Professor Guthrie, whom I had the honour once of having as an assistant in Edinburgh, before he became a distinguished Professor in London, is now going to show you the grand experiment of Torricelli, which led us to everything we know about the physical quantities of air. Torricelli said if it is true that it is the weight of the atmosphere which supports a column of water 32 or 33 feet high, mercury, which is 13 times heavier, will not go nearly so high, it will only go 30 inches; and if water can be supported 33 feet, mercury could only be supported 30 inches. Now, Professor Guthrie has filled a tube more than 30 inches long, and when he turns it over into a little mercury you see it only stands 30 inches high in the tube, the height of the barometer of the day; observe that there is a vacuous space above it, because the pressure of the atmosphere will not keep it up more than 30 inches. That first experiment of Torricelli's was the master experiment which led us to everything we know about the physical condition of air. It completely proved Galileo's view about the 33 feet of water, and it gave us not only, for the first time, a barometer, but it gave us a vacuum free from air. I am going to make no experiments that every boy does not know, because my lecture is not on the atmosphere, but on the history of the atmosphere, and I am only about to repeat the old experiments which were made

long ago. Here is a balance; to one side is hung a flask which contains air, and it is exactly counterpoised. Now, we will pump out the air, and you see at once the air has weight. A few strokes of the air pump has partially exhausted this flask, and you will find on weighing it again that it is lighter; you see now, although only a grain or two has been taken out of it, it is distinctly lighter; but now we will open the cock and let the air in, and it goes back again to its original position. Therefore, unquestionably, by experiment, air has weight.

Now what weight has air? The weight of a square inch of air reaching from the surface of the earth to the top of the atmosphere is about fifteen pounds, or thirty-three feet of water on a square inch weighs fifteen pounds, or a column of mercury of thirty inches on a square inch weighs also fifteen pounds. Therefore you know what the pressure of the atmosphere is. That is an envelope all round the globe; and there is such a prodigious quantity of it, that, if you could take all the air round the earth and weigh it, you would require to put into the opposite balance a solid globe of lead sixty miles in diameter.

Now I come to a critical point in the history of air. Torricelli's experiment was the most beautiful of all; but then came a man who gave an immense impulse to all discoveries with regard to air by the popular way in which he drew attention to it rather than by any remarkable discoveries of his own. His name was Otto de Guericke. He was originally an engineer, and afterwards became burgomaster of his ancient town, Magdeburg. He made so many curious experiments about air that his fame spread all over the world. He thought he would try and make an air-pump to pump out air just as we pump out water, and here is the original book which he published. It is, as all learned books at that time were, written in Latin, but it is extremely interesting. It is a very rare book; I know of only one other existing in the Bodleian Library. Here is the engraving showing the way in which he tried to obtain a vacuum. He took a stout barrel and filled it with water, and observe in the picture how the old gentleman is trying to pump the water out of the barrel without letting in any air. He thought he would get at a vacuum in that

way; and there he is working away at the pump, pumping the water out of the barrel hoping to exclude air. But he had not thought of the tremendous pressure of the atmosphere; the air went squeezing in through every pore of the wood and through every joint, and filled the barrel quite as fast with air as he took out the water. That beat him. Then he was much surprised to find that he could pump out air just as easily as he could pump out water by making an air-pump, and here is actually Guericke's original air-pump, the first ever made in the world. Scientific men might worship a fine old instrument like that, glorious in the history of science, quite as much as a devotee would the bones of any old saint. From the air-pump there came the celebrated experiments of the Magdeburg hemispheres. I will show you the originals presently, but here is a model which my friend Professor Guthrie will help us to exhibit. It is a globe cut into two parts so as to be hemispheres, and they fit closely together. We can pump the air out of them, and then you will see the experiment which when made by Otto de Guericke astonished the whole world and spread like wildfire the properties of air so as to set everybody experimenting about it. When we pump out the air from the inside, the atmosphere will act with enormous pressure on each side of it, and here you will see how much weight you can attach without separating the two hemispheres. These heavy weights are not able to pull the two parts asunder, because the atmosphere is pressing on the globe; but the moment air is allowed to enter, then the pressure ceases, and they fall separate. Now what I have shown you is a mere toy, but here are the fine old instruments which were my dream in science years ago, though I never thought I should actually touch or handle these wonderful hemispheres. They are described in every scientific book, and are well known to every man of science.

These then are the actual hemispheres which Guericke used before the Imperial Diet, at Ratisbon, to show the same experiment on a large scale which we have just shown you on a small scale; and here, in this same book you will see how astonished the Emperor and all the Princes at the Imperial Diet were when Otto de Guericke pumped the air out from these hemispheres exactly as you saw

done just now, and attached eight horses to each side. Here are the old traces with which the horses were harnessed. They were attached to each side of the hemispheres, and he lashed away at them, but the sixteen horses could not pull them apart. The Emperor and the Diet looked on with intense astonishment at this remarkable experiment, and very remarkable it was at the time, for nobody knew the enormous pressure of the atmosphere.

The old philosopher made one slight mistake. If he had fastened one side of the hemisphere to a solid wall and put eight horses to pull at the other side, he would have had just as much force; but, however, sixteen horses looked better than eight, and he employed sixteen. Otto de Guericke was a wonderful old fellow; he made also a water barometer which astonished everybody; it was thirty-three feet high, above which there was a vacuum, and a little man floating at the top; and whenever the little man bobbed up, it was known it was going to be fine weather, and when he sank down, it was known it was going to be bad, and this made the barometer very popular.

I am now going to pass away from our physical notions of the atmosphere to our chemical notions. All this time the atmosphere is an element, but it was not until after 1774 that its elemental character was doubted; and I want to tell you how we got our knowledge of the chemical properties of the atmosphere. The first person who added to our knowledge of the chemical properties of the atmosphere was a very celebrated chemist, who wrote a great work called 'The Sceptical Chemist' — Boyle. Boyle, in fact, was one of the few members of the aristocracy who have added much to science, because their education is generally classical and not scientific. Boyle has been called the "father of modern chemistry and the brother of the Earl of Cork," and he added largely to our knowledge of air. He showed that there were different fictitious kinds of air, as he called them. He extracted airs from different substances and showed that they were peculiar, but he never thoroughly distinguished them from common air. They might have been common air with impurities; he never went far enough to make it quite cer-

tain that they were separate entities. Some are inclined to give Boyle more credit than I am now giving him, but I have read his essay attentively, and I do not think that he knew clearly that there was any other air than common air. The proof of this is that shortly after him came a celebrated chemist, Hales, who wrote a famous essay in which he described how he had made a great many airs which we know now to be separate airs, for it is undoubted that Hales made hydrogen, chlorine, carbonic acid, and nitrogen, but he did not believe them to be anything but common air; he said they were airs tainted or infected with fumes of acid and sulphurous spirits. Although he had actually made airs which we now know to be elements, or compound airs, he did not believe them to be distinct airs, because the whole world believed air to be an element; they were all trying to get the impurities out of these airs, which they thought gave them different properties. Hales made very remarkable experiments, and his essays give us a great deal of information with regard to airs.

Now I come to a man for whom I have a great veneration. He is a sort of scientific ancestor of mine, not by blood but by professorship; he was three before me in the chair of chemistry in the University of Edinburgh, and made grand discoveries—very few of them, but those he did make were grand. I allude to Professor Black, and he was the first man that gave to the word air a plural. Chemists knew of air, and sometimes called it gas, but nobody knew that there were airs or gases, and Black was the first man that gave the plural to the idea of air. Hitherto all chemists had been examining into the qualities of bodies, but since the time of Alhazen nobody had been estimating their quantities by applying the balance to them. Nowadays every operation of a chemist is done by the balance. Up to the time of Black, men had forgotten the balance. They had speculated about the qualities of substances, but they had not done anything with regard to quantities. Black proceeded, as Alhazen had done in 1100, with a balance in his hand, and tried everything by its means; and that venerable balance, which is now the charter of almost all chemical research, is this grocer sort of

balance which you see on the table. It is no better than a pair of grocer's scales, but it is the father of all modern balances. Here is a modern one in which we can turn it with the thousandth of a grain; but this rough specimen was a most venerable balance in the history of chemistry. When I succeeded to the chair of chemistry in Edinburgh I found this venerable balance in a lumber room, and immediately sent it to be deposited as a sacred thing in one of our public museums. I am now going to tell you what that balance did, because it led to all our knowledge with regard to airs. Hales had completely proved that air was a constituent of substances, that there were many airs in substances. That had been known long before in the world. Solomon knew it. Do you not recollect a verse in which he says, that speaking to an angry person is like pouring vinegar on nitre? That is nonsense, because if you pour vinegar on nitre nothing happens; but the word "nitre" there is a mistranslation for the word "natron," which means carbonate of soda; and if you pour vinegar on soda you know what takes place, it effervesces and fizzes from emitting a constituent of air. Black began to examine these substances, balance in hand. At the age of 23 or 24, when he was just coming out as a medical student, he read a paper on the difference between mild lime and burnt lime. You know that mild lime is limestone, and the latter put in kilns and burnt becomes caustic lime, which we use for mortar. The theory at the time was this. The principle of fire was then called phlogiston, and everybody thought that this phlogiston had an immense influence upon matter. Lime becomes caustic, because it was burnt in the fire and phlogiston entered into it. Black said if phlogiston enters into it, it must become heavier, or, at all events, if my balance does not tell me that it becomes heavier, it cannot positively be lighter, if anything goes into it; therefore, he took a bit of lime, weighed it with that crude balance, and burnt it, and he found that instead of the phlogiston rendering it any heavier it was a good deal lighter. So he said, I must have burnt some air out of it. He left his caustic lime upon the balance, and as there is always carbonic acid, which he had driven out of it, diffused in the air, the caustic lime began to absorb that carbonic

acid from the air and to get heavier and heavier. Then he said, it must absorb some air which is in common air out of the atmosphere or it would not get heavier again; and then he tried an experiment, which was to take a certain quantity of limestone, such as I have here in the form of chalk, and having weighed a quantity of acid, he added it to the chalk. That effervesces, and the effervescence is due to the formation of carbonic acid. He weighed it again, and he found the diminution of weight was exactly the same as when he burnt it. The strong acid here drives off the weak acid, the carbonic acid, and in weighing it he found the loss was exactly the same as it lost in being burnt. He had now the key to the whole thing. He said mild lime differs from caustic lime by combining with air. It was not known what the air was, and he did not describe the air in the original paper he wrote. But I have seen many of the notes of students taken during his lectures, and it appears that he well knew what the air was, though he did not describe it in the original paper. He tells you that the air that comes out is the same air that men exspire when they breathe, for we breathe out carbonic acid in our expiration from the lungs. He says it is the same as comes from decaying substances, and from brewing beer; so that he knew the air very well, though he did not describe it. It came afterwards to another great philosopher to describe it; but before we part from Dr. Black, I have such a great love for the old philosopher that one cannot help regretting that his wonderful masculine mind and power of researeh produced so little. He was about 24 when he published his first paper, and he was 34 when he published his great researches on latent heat, and explained how it was that different kinds of matter have different forms, because they have different quantities of latent heat. He lived to the age of 80 and published no more. He was a man of delicate health and was fond of quiet. He did not like the controversies which were raging at that time, chiefly as to whether phlogiston had so much to do with matter as philosophers supposed, and he got out of those controversies and did not publish new researches. But while we regret that a grand intellect like his, which made two such great discoveries as the identification of different airs and latent

heat, added nothing more to science, we must recollect that when he did walk in pastures new he always left the impress of his manly tread, so it is not for us to complain who cannot climb up even the boundary wall to see over into those pastures. I should tell you that old Black died in a beautiful way. He was found in his study, sitting in an arm-chair, with a book in one hand and a cupful of milk resting on his knee; and when the servants came in they found him dead, and not a drop of milk was spilt, as if he wanted to show, by a last experiment, how a good man could die.

We come now to the time of Cavendish. Cavendish took this air of Black's and examined it thoroughly. Here we have some of this air which we call carbonic acid, and you will see how much heavier it is than common air. We have this glass vessel full of common air exactly balanced; Professor Guthrie will now pour some of this heavier air into it, and you see it cannot be common air, because it weighs down the balance, therefore it is much heavier than common air. We can also show you that there is another air which Cavendish discovered, coming when metals are dissolved in acids, and which we know as hydrogen, but which people at that time thought was genuine real phlogiston. In order to weigh this you must turn the glass vessel upside down, because hydrogen is much lighter than common air. On filling the glass vessel with this hydrogen, the other side of the balance goes down because the lighter air is being introduced. Cavendish made an immense stride with the balance, as you will see if you look at Black's balance, and at Cavendish's original balance, which is here. Cavendish was another instance of a noble family who did a great deal for science. He was one of the purest intellectual lights that science has ever had. He was scarcely a man of feeling; there was nothing grand or noble or chivalrous in his character, but there was nothing selfish or ignoble in it; he was a pure sun which poured light down upon the world without much warmth, but he, from his intellectual discoveries, advanced philosophy very greatly. This is the balance with which he took the specific gravities of different kinds of air. He knew a number

of airs. He found hydrogen, but he did not describe it, and he made this capital observation which has made chemistry progress more than any other discovery which has ever been made since the world began, that hydrogen on being burnt produces water. Oxygen and hydrogen form water, and that was Cavendish's experiment. There is his balance which has almost all the elements of the modern balance in it, although in a somewhat rude condition. It is a great advance on Black's.

In 1772 a new gas was discovered in air, but you must remember that oxygen was not yet discovered; in 1772 people were still doubting whether air was an element, they had not proved it; but Rutherford in 1772 found nitrogen. His mode of making it would have horrified, I think, the anti-vivisectors of our day. For he took a jar of air, placed it over water, and put in a mouse, and let the mouse remain there until it died, then he washed the air with lime-water, and the lime-water took away all the carbonic acid the mouse had produced in breathing. Then he put in another mouse and let that die, and so he went on until the last mouse died the moment it was put in. Then, what was left was nitrogen, because the mouse had taken away all the oxygen, and breathed it away in the form of carbonic acid, which Rutherford had washed out with lime-water, so that what was left was nitrogen. Nitrogen forms three-fourths of the air, and the other fourth is oxygen. Nitrogen is a substance which is admirably fitted for its purpose. Oxygen is the great lover of the chemical elements. It tries to marry all the other elements, to unite with them whenever it can; but, nitrogen is the bachelor of the chemical elements, and will not enter into union with them without great compulsion. In the atmosphere, therefore, it is useful to form a diluent, to give us all the properties of air without imparting any active chemical properties. It was not known that it was such an important substance when Rutherford discovered it.

Now I come to the year 1774, which is a capital year for chemistry. That is the year when Priestley discovered oxygen, and that nearly completed our knowledge—not all our knowledge, for our knowledge is still very imperfect with regard to air,

but it made an enormous stride. He found that there was a substance in air, oxygen, which could be separated, and he learnt how to make it separate. He found it had a wonderful power over combustion; for instance, a light goes out immediately in carbonic acid, or in nitrogen, but oxygen starts a spark into light again immediately. Priestley examined this substance so peculiar in its character, which has so wonderful a power over combustion, and that gave us our existing knowledge of air. Air then, with the exception of some constituents, which I shall refer to presently, consists of nitrogen which will not support combustion at all, and oxygen which it is mixed with. Priestley was a dissenting minister in Birmingham. He was a man far in advance of his time in every way. He had a habit of saying what he believed, both in religion, in politics, and in science, and so he led a most turbulent life. He was prosecuted by the Government, by the Church, and by the people, who pulled down his house, burnt his library and instruments, and he was obliged to fly to America; and yet Priestley is one of the scientific men whose name must always stand highest in honour in this country. Priestley went to Paris in 1774, and showed the great Lavoisier how to make oxygen. Lavoisier is the founder of modern chemistry, but he never confessed that Priestley had shown him how to make oxygen. He saw at once that oxygen was such an important thing that it would revolutionise all chemistry, and make things explicable that were not known before, and immediately began to work with it. He founded our modern chemistry, and after a few years he renounced oxygen as his own discovery, but he never said that Priestley had shown him how to make it. Lavoisier was a wonderful man, though not so great as a discoverer; but, like Plato, he could see the one in the many, and the many in the one. He was a great generalizer, and could explain phenomena in a wonderful way. Here is the actual instrument with which Lavoisier made his celebrated researches on combustion. It is called the calorimeter. In this he burnt substances in oxygen, and weighed them in air. Lavoisier always used a balance, as Black had taught him in his researches on lime. He surrounded the combustible with ice, and found how

much ice was melted, and so measured the combustion by heat. The water ran out of this stop-cock into a measured vessel, and so he was able to measure the quantity of heat produced by the combustion of various substances. He completely proved that there was no such thing as phlogiston, but that combustion was simply the union of combustible matter with the oxygen of the atmosphere, and the chemical phenomena were those resulting from the affinity of oxygen with the burning substance. Lavoisier was another of those unhappy men who came on troublesome times. He lived in the time of the revolution, and his head was cut off by the guillotine when he was in the very prime of manhood, and at the very height of his discoveries. Afterwards his wife Madame Lavoisier, dressed as a priestess, in solemn procession took the theory of phlogiston which Lavoisier demolished, and to which Black had given such a great blow by the use of the balance, in experimenting on lime, and burnt this theory of phlogiston on an altar, having a solemn requiem sung for its soul. So the French thought that old Mother Chemistry was buried on this occasion, and that the new chemistry was a new child born of Mr. and Madame Lavoisier, who was for ever afterwards to reign over our chemical science, and she certainly reigned superbly for a time. But now like all persons she is getting a little old; her teeth are decaying, her skin is shrivelled, she is still with us as a venerated ancient, but her time has almost gone by.

Now what do you think of all these errors that occurred in acquiring our knowledge? I think you have listened to but little purpose if you contemn theory, because you see theories appearing and disappearing. You see the phlogiston period, and you see modern chemistry at the present moment ageing fast, for the French Chemistry will no longer serve our purpose. But the theory of to-day is the error of to-morrow. The error is nothing but a shadow cast by the strong light of truth. Theories are an absolute necessity to the man of science, because they collect in one focus all the knowledge which he possesses at a time. Theories are the leaves of the tree of science that draw nutriment to the parent stem, and when the leaves fall off they are not

lost; they fall to the roots, decay, and give vigour to the stem and to the branches, and it is out of their own materials that the new leaves, or the new theories reappear. A theory that disappears is like a phœnix which is destroyed, but that rises glorious again from its ashes. The theory of phlogiston, of this inflammable matter which was in every substance, which has exerted so much influence in chemistry, is reappearing. We no longer call it phlogiston; we call it potential energy. What chemists tried to find as a spirit or a substance, we now know to be a potential energy produced when there is chemical combination, and that which they called phlogiston we now call potential energy.

Now I must sum up very rapidly. All that we know of the air is pretty much this. It consists mainly of oxygen and nitrogen and carbonic acid. That was air when I studied, as many of my young friends are studying now. That was what we were taught when the late distinguished Professor Graham was my professor and master in Glasgow; but a number of nice little strangers have since been born into the air during my own lifetime. For instance, ammonia was found in air. Ammonia, you know as smelling-salts, but that it exists in the air was not detected till modern times, when I was a student under Professor Liebig in Giessen. But the reason was that it is very soluble in water, and the rain washes it out of the air. Liebig thought of looking for it in rain, and he found ammonia in the rain. Since then other substances have been found. For instance, there is the peculiar substance known as ozone, which is the active form of oxygen, and very useful for attacking vaporous foul matter and burning it up. Then there are a great number of minute little organisms, having very important functions in promoting decay and putrefaction, and even in generating disease. All these interesting little strangers have been born in my own time. So who can dare to say that we have nearly come to the end of our knowledge of the air? We are gradually going on with our knowledge, and every few years something more is discovered with regard to air. We now know that the connection which air has with plants and animals is an extremely interesting one, and with that I will finish. How is it that the air into which so much foul matter passes, by the respiration of

animals and by the decay of organic matter, is kept in a state of purity? We are continually burning coal. The oxygen unites with the carbon it burns, and the carbonic acid passes into the atmosphere. We excavate annually in this country about 90 million tons of coal, and that produces 280 million tons of carbonic acid, and all that choke-damp is constantly going into the atmosphere. Then our own lungs are continually expiring this carbonic acid; substances are putrefying; all organic matter putrefies and decomposes into carbonic acid—water and ammonia, and this passes into the atmosphere. What becomes of the atmosphere, the balmy atmosphere of the poets? It now would appear to you to be a huge sewer into which all the pestilential matter goes. What prevents that foulness? It is simply the wonderful arrangement which has been made between plants and animals. Plants live on these substances which are the product of decay and life of animals. The carbonic acid which we give out from our lungs and by our habits of civilisation in the burning of coal, and which emanates also from dead bodies, and from all foul matter, passes into the atmosphere, and plants mould it into the forms of organic life. They extract the oxygen from the carbonic acid, and restore that oxygen to the atmosphere; and there is the wonderful system of winds which helps this action. In our cold climates we burn coal and produce a large quantity of carbonic acid, but a current of air, low upon the earth, carries that to the equator, under the name of trade winds, where there is a constant and ever-glowing sun acting upon a luxuriant vegetation, and that vegetation decomposes this carbonic acid, and moulds the carbon into organic forms, giving back the pure oxygen to the atmosphere. So this is the wonderful circle connecting plants and animals; for plants feed animals, and animals equally feed plants. Although plants go for a moment into animals, the whole life of an animal consists in the unceasing death of its particles. When I move my arm, a part of it is turned into carbonic acid, and the carbonic acid ultimately passes from the mouth, and therefore there is a death of particles at every moment. By every thought in the brain

there is a death of a particle of the brain, which partly passes out as carbonic acid from the lungs, and as amido-carbonic acid (urea) in the urine. So plants take that carbonic acid and feed upon it, and give back oxygen to the air, and in this way they keep the atmosphere in a state of perfect purity. The oxygen, for instance, which we inhale to-day for the purpose of our life may have been distilled for us by the great trees that skirt the Orinoco or the Amazon; the oxygen formed by the glowing sun in the tropics may be breathed out by the roses and myrtles of Cashmere, or by the cinnamon trees of Japan. There is not a blade of grass or an animal too much in the world; one balances the other, one feeds upon the other, and each is necesssary to the existence of the other.

To what then do we come back? We have really come back to the old ideas of the ancients, very much modified by the conceptions that have grown around us. You are not surprised now that Diogenes of Apollonia, or Anaximenes, thought that life was intimately connected with air. Life is wholly connected with air, and the essence of a man's living is the condition of his being able to have pure air. This intimacy of life between plants and animals is the old notion of Anaximenes and Diogenes of Apollonia come back in another form, the unassisted senses now heightened and brightened by the conceptions which we have inherited from ages, not from one man, but from a whole series of philosophers. We have no longer the fetish spirit in the air which Diogenes of Apollonia saw and worshipped, but we have air vivified by a star which is nearly 100 millions of miles from us, the sun; and it is that sun coming down upon the earth, in that great act of creation, when there was said, "Let there be Light, and there was Light," which enables plants to become the laboratory of organic life. It is quite true that air is the grave of all organic life for we all pass into air when we die. But the air, also, is the cradle of organic life, for it is from air that all life proceeds through this vivifying action of the sun, which enables it to be moulded into all the various organic forms. So we find that this light, the nature of which the ancients did not know as we know it, is the source of nearly all our power on earth, and is the

power which maintains the marvellous balance of organic life in the world.

I thank you for the attention you have paid to the lecture.

The CHAIRMAN: Ladies and Gentlemen,—Dr. Playfair concluded his most interesting lecture by thanking you very much for the attention with which you had listened to him. I am sure that your feelings, like mine, are those rather of gratitude to him for his kindness in coming forward this day. Yet when I saw him at work, and saw how kindly he took to it, I could not help feeling that he regretted, perhaps, the happy days when he was a Professor at Edinburgh, and, perhaps, almost wished to be there again. And when he spoke with gratitude of the great men under whom he had studied, and from whom he had derived his very extensive knowledge, I am sure it must also have been a satisfactory reflection to him that his mantle as Secretary of the Science Department has fallen upon such worthy shoulders as those of Major Donnelly. In thanking Dr. Playfair we cannot but be, I think, most grateful to that Department of Her Majesty's Government which has provided us with the means of the interesting instruction we have received to-night, for, but for the action of Her Majesty's Government, at the suggestion, I believe, of the gentleman who succeeded Dr. Playfair, we should not have had here the means of seeing the gradual steps by which science emerges from a state of utter darkness into a state of comparative—I dare not say, after what we have heard, of complete—light. But we have here heard descriptions of the earliest action of men's thoughts; we have seen before our very eyes the earliest instruments by which effect was given to those thoughts. You have had them admirably explained to day, and it is your fault if you do not go from this room wiser than you entered it, and thinking that what is popularly called "the "common air" in future is something very uncommon, and well worthy the investigation of philosophers and the attention of thoughtful men.

ON DAVY'S AND FARADAY'S APPARATUS.

By Professor Gladstone, F.R.S.

July 8th, 1876.

Macleod of Macleod in the Chair.

The Chairman: Ladies and Gentlemen,—I have the pleasure of introducing to your notice Dr. Gladstone, F.R.S., who has been good enough to undertake to deliver a lecture to you this evening, for which, I am sure, we are all very much indebted to him.

Dr. Gladstone, F.R.S.: Ladies and Gentlemen,— If leaving this building we walk down Piccadilly, we shall find a quiet street on the left hand, called Albemarle Street. Proceeding down it we may observe 14 pilasters and a Grecian façade on a level with the other houses. This indicates the Royal Institution. Entering it by the swinging doors we find ourselves in a vestibule from which rooms open right and left, and there is a staircase in front which leads to a library, and also to the lecture theatre. In these different rooms there is a large collection of books: in this lecture theatre we find a horse-shoe table, and many rows of semi-circular seats round about it, and a capacious gallery. If we go below we shall find at present two laboratories, the upper one the physical, and under it the chemical laboratory. If we had gone there at the commencement of this century, during the first ten years, say, we should not have seen that Grecian façade, but we should have found the building inside essentially the same. The laboratory, however, was somewhat different, because it was one underground room, attached to which was another small lecture theatre. It was not so well lighted as the lower room at present is, and attached to it there

were two or three very dim rooms that had been really the cellars of those houses. I want, however, to introduce you not so much to the building itself as to the work which has taken place in it. If at the time I mentioned—the early part of this century—we had found our way there we should have observed in all probability a considerable portion of the fashionable world wending their way thither also, and thronging into this lecture theatre. We should have seen it filled with the *élite* of society listening with the greatest attention to lectures delivered by a young man who was then rising into great fame. He had been born in Cornwall, and had afterwards graduated in science (if I may so speak) at Clifton, where he had acquired renown as an investigator into nature, and then being brought up by Count Rumford to London, Davy had become first of all assistant, and afterwards Professor of Chemistry in the Royal Institution. There seems to have been some strange charm about the young man's enthusiasm and his way of lecturing that could draw this fashionable audience there time after time. It is true that there were very few other institutions in which science was treated; few schools in which it was taught; no museums like this in which could be seen splendid collections illustrating the advance of science; but at that place, which was founded for the diffusion of knowledge and for the application of science to the common purposes of life, these enthusiastic audiences were collected together whenever Davy was announced to lecture.

But it was not Davy as a lecturer only, but Davy as an experimenter that drew persons there. When he first came to the Institution, Volta had just introduced his battery, and exhibited this strange form of electricity that was found capable of tearing asunder water and many other compounds; and Davy seems to have endeavoured by all the means in his power to tear asunder as many compounds as he possibly could.

I am able to bring before you the five troughs and batteries which he employed in most of these experiments. These are the very batteries, belonging now to the Royal Institution, but lent to this Loan Collection. They are worn out, corroded

with hard work, and I should not like to fill them with acid, or try to work with them, because I fear they would work no longer. Anything, therefore, I may show you will be with a modern battery instead of this one. However, we may look with great respect on these five troughs, knowing that with these Davy was able, among other things, to decompose the alkaline earths, and to show for the first time the metals potassium and sodium. As this was one of his most important discoveries, I will endeavour to repeat it before you. I shall take a little potash in this spoon, and heat it. I have now a little of it fused. I will call for the lights to be put down, in order that you may see something of this metal potassium. Of course, there will be a very little produced in this way, but still I hope you will be able to see that it catches fire when coming into the air when hot. You may now perceive where I place this platinum wire into the fused potash, the potash is decomposed and the metallic potassium is thrown off in globules, but directly these come into the air they catch fire. I am sorry that this is obliged to be on such a small scale, because we cannot here catch the products. I will show you, however, the metals potassium and sodium, and another compound which belongs to the same group. I will take the metal potassium. Potassium is a body which is now made by a different process to that I just now showed you, and is prepared on a very extensive scale. It cuts more easily than lead would cut: it has somewhat the same appearance as that metal, but it is exceedingly light, although when Davy discovered it he says that one of his friends, to whom he showed it first, took it into his hand and remarking how like it was to lead, added, "Oh! how heavy it is," until Davy showed him that when placed on water it would float, instead of sinking. I will not put it on water, but will place it upon a piece of ice. You see it attacks water so readily that it catches fire on the cold ice and burns away. No wonder it caught fire when it was produced over that hot spirit lamp in the air.

I will now take the other metal sodium, which can be produced from soda. I prefer throwing that upon hot water. If I were to throw it upon cold water, the probability is that

it would just roll about, and gradually decompose. But instead of cold I will take hot water, and the heat will make all the difference, I hope. You remember perhaps the intense purple flame of the other metal. This one has now caught fire and burns with a yellow flame. This then is the metal sodium.

Then there was another alkali which puzzled Davy very much. He thought that the volatile alkali ought to give him a similar result. He took this volatile alkali, ammonia, or spirits of hartshorn, and worked away at it considerably, and so did others imitating his example; and they found that when a salt of this ammonia was decomposed, in the presence of mercury, the mercury swelled up and became somewhat buttery in character, and it seemed as if there was some new metal combined with the mercury. This therefore was called the ammoniacal amalgam. I will not show you that on the small scale on which alone we can produce it by means of the battery; but we will take another amalgam and show you the result of a substitution. Here I have some sodium amalgam. The sodium will displace the ammonium, when I throw it into this solution of sal ammoniac. We shall find the mercury begin to swell. I think you will now see that the little I placed in the glass has enormously increased in quantity and changed in appearance.

Well, this may not be a compound of a *quasi* metal, ammonium, with the mercury, but if it is not that, it is a compound of hydrogen with the mercury. At present it is giving off not only ammonia but hydrogen, which causes all this bubbling and this cauliflower appearance, and these vesicles which are rapidly produced in it. It will soon be reduced again to metallic mercury.

Davy, working with these metals, was able to decompose many of the earths and other substances so as to get other metals, metals perhaps not quite so capable of decomposing water as potassium and sodium, but still metals which had a great affinity for the oxygen of the air.

Barium was one of these; and magnesium was another. Magnesium, as it happens, has become a substance of some importance since in various ways. Here is a little magnesium lamp.

We can set fire to the metal and you can see it burning, and observe what an intense and peculiar light we get from this magnesium which is one of the metals which Davy was the first to produce.

Davy was not content simply with showing these new bodies—these alkali metals—he wanted to show how potash and soda were really built up; and, therefore, he made experiments in various ways. He proved at length that potash really contained both this metal potassium, or alkaligen as he called it first, and oxygen. Now we have before us one of his note-books, in which he recorded his experiments. It is his laboratory note-book, and it is placed here in the Loan Collection. In order to show the way in which Davy worked, I will read to you just this page. The writing is not very clear, but as I have read it before, I dare say I can read it now without stumbling. "When potash was introduced into a tube having a platina wire attached to it, so," (then a little drawing), "and fused into the tube so as to be a conductor, that is, so as to contain just water enough, though solid—and inserted over mercury, when the platina was made negative no gas was formed, and the mercury became oxydated and a small quantity of alkaligen," as he called potassium, "was produced round the platina wire." Then he says, "When the mercury was made the negative, gas was developed in great quantities from the positive wire, and none from the negative mercury, and this gas proved to be pure oxygen. Capital experiment!" he writes at the bottom, and a capital experiment it was. Well, this was one of the researches of Davy, which I am able to illustrate by the apparatus belonging to the Royal Institution which is now exhibited in the Loan Collection over the way. I must not take you through all his various researches. I might speak of nitrous oxide or laughing gas, for one of the finest researches he ever made was on that body. I have laughing gas in abundance upon the table, but I want to use it presently for another purpose. I might speak to you of how he showed the nature of chlorine. That was another of his most brilliant discoveries, and I have chlorine also in this box, and I shall presently refer to it in speaking of Faraday.

Since I have come into the room Professor Eccher has reminded me that in the collection over the way there is the large burning glass from Florence, the lens that was used by Davy for the combustion of diamonds. In the focus of that lens he put the diamond, and the sun of Italy streamed down upon it, and caused it to burn. In this way he discovered that diamond was made of pure carbon.

I will hasten on to another of Davy's discoveries. You may say that these to which I have already alluded have a very remote bearing upon the actual requirements of man. But the one which I shall speak of now has a direct bearing on the welfare of our fellow-creatures. In 1815 the attention of Davy was very forcibly directed to the accidents in coal mines from the explosion of gas. This seems to have taken possession of his mind, and he wanted to find some way in which it might be cured. It was in Scotland he thought of it. He returned by Newcastle and saw some mines there, and went vigorously to work in London. He was a man who entered heartily into things for the benefit of his fellow-men, and during that autumn he was able to produce such good work that he invented the beautiful contrivance which is known as the Davy lamp.

I will show you the principle of the Davy lamp first of all. I will not take coal gas first but an ordinary candle, and I will show you that there are more ways than one of extinguishing a candle. It is possible to extinguish it by putting a little coil of wire over it. That puts the candle out perfectly, although it does not obstruct the air in any way. It was merely because it was cold, and conducted away the heat of the flame. We may light the candle again and heat our wire, and then we shall find, I think, that it will no longer extinguish the candle. I will just place it upon the flame. You see the light burns still, although the wire is round it just as before, because it is now not cooled down, and therefore it continues to burn.

This shows you the immense importance for the continuance of a flame, that there should not be metal or anything else about it which carries away the heat. But the coal gas itself—or the gas of the mines which is almost identical with the gas we are burning here—is

a substance much less disposed to burn than the candle is; and it was found that an explosive mixture of air and the fire-damp of mines when caused to pass through tubes—these are some of the early specimens that Davy used—will not explode. We will place this apparatus, consisting of small glass tubes passing through a cork and secured by sealing wax, on the top of a tube through which gas mixed with air is passing, and we shall see that on setting fire to the gas, it burns away very quietly at the end of the tubes, but the fire does not run back through the tube, although there is an explosive mixture below. Now, instead of these tubes we will try another experiment. A number of little plates of metal are put side by side like an elongated gridiron, if I may say so, and the gas will pass through these and will burn at the top just as it did through the tubes. There it burns, and it will continue to burn any length of time. Davy took advantage of this principle, and at first he made the kind of lamp which I have here. He started with the idea of lanterns. This piece of apparatus which I showed you first, he put in the bottom of his lantern, so that the air would go up through these tubes. Now there is a flame in the lantern, and the flame cannot come down through these tubes, and therefore if there were an explosive mixture sweeping through the passage of the mine, and going through these tubes and supplying this lantern, the flame could not ignite the gas outside. Then in the upper portion he had another arrangement. He put some of the tubes in here, so as to cool down the gas as it came out and passed away. Thus he was able to produce a little lantern which was safe in the midst of explosive gases. Then he made other forms. Here are several. There are metallic tubes and there are glass tubes. In this way he was able to manage to burn a feeble light. The flame must have been rather bad—smoky; but still it gave light to the miner. Here is a lamp which I can hardly imagine did succeed, since there is a great want of ventilation, and though quite safe, I doubt if it could give much light.

I want to pass on now to what was really his best discovery, as far as the safety lamp was concerned. He actually introduced these lamps which I have shown you, into the workings of coal

mines with success, but he improved upon them afterwards, by finding that if he took sufficient cooling surface he might reduce his tubes to something exceedingly small.

We will take wire gauze, rather thick, and we shall find that we have the mastery of a flame. We will take a large flame, and I think it will pass through this large specimen of wire gauze, because the meshes are very wide; but you see even that affects the colour of the flame very much. This gauze again is much finer, and the flame cannot get through it. If we put it at the top of a tube through which gas is issuing, we can light the gas on the upper surface, but the flame will not pass through. I can lift the gauze, and the flame with it, a considerable height; in fact, until the flame goes out; and it would only catch fire underneath by the flame getting round the edges.

I want to show you that in another form. You may remember this tube I experimented with just now; it was filled with explosive gas, and I will fill it again. Inside this tube there is a little bit of wire gauze stretched across it: the flame will run down and stop there, producing a musical note. In that way we get a complete mastery of the flame. I have taken a piece of wire gauze, and just twisted it up in the form of a filter. I will light a little turpentine, and there, you see, is a very large flame. I pour the turpentine into this gauze, and you see it runs through fast enough, but the flame does not; it cannot get past the wire.

I think I must show you one pretty little device which was shown me at the Loan Exhibition the other day by Dr. Mann. He said he meant to exhibit it some time or another, but he was quite willing that I should show it here also. There we have the gas; it mixes with air and passes through a little wire gauze, and those of you who are near will be able to see this beautiful blue cone inside. The fact is, that the mixture has to be heated up to a certain temperature before it catches fire, and this cone is a hollow cone of explosive gas that catches fire just where it becomes hot enough. All outside that inner cone is what is called a solid mass of flame. Here is a little piece of platinum wire which I will put through the flame above the cone, and you

see the whole of the piece of metal becomes incandescent or red hot, showing that the flame stretches wholly across.

These are all illustrations of this great principle, that a mixture of gas and air will not inflame except at a pretty considerable temperature; and if we reduce the temperature, by causing it to pass through narrow tubes, or wire gauze, it will not convey the flame; it will only burn on one side of the obstacle.

Now when Davy perceived that, he improved upon his safety lamp. He said if we simply surround the flame with pieces of wire gauze, it is all that we want, because air will pass through the wire gauze below, and the products of combustion will go away above. Therefore he made his Davy lamps of this very simple construction; just a little piece of wire gauze round the flame. Here are various arrangements, some square and others round; some with exceedingly coarse wire gauze, and others extremely fine. I believe I have here the very first Davy lamp that was ever made. It is a small piece of very fine wire gauze indeed; there is a little wick to be lighted, and no doubt Davy found that he could put the protected wick into an explosive mixture without igniting it. There are various other forms. Sometimes he employed perforated metal, sometimes wire coiled round and round tightly; and he appears to have tried a great many different expedients before he adopted a permanent form. This perhaps is the most typical Davy lamp. There is a lamp with its wick encased in this column of wire gauze, which is doubled in the upper part because there it gets very hot; and when it is red hot there is a certain danger of its igniting the coal gas. Here is one with a lens in front. It is protected in the same way, the products of combustion passing through the wire gauze. There is a very fine collection in the Exhibition, near the entrance at Queen's Gate, of many modern forms of safety lamp, besides these specimens of Davy's. I will pass this lamp now into a highly explosive mixture. You see as it goes down, the flame changes in appearance. The miner would know he was in a dangerous atmosphere, but, still there is the lamp burning, though no explosion takes place. It has become very much dimmed now, and it will eventually be put out by the explosive mixture instead of the explosive mixture catching fire.

We see in that way how the scientific researches of Davy were productive of one of the most important discoveries for the benefit of his fellow-men, so that they could work in those underground passages with perfect impunity from the dangers of the explosive fire-damp. We know unfortunately that accidents still occur. When gas does explode, I believe it generally happens through the carelessness of the workmen, who will open their lamps in order to obtain more light.

Time is passing very rapidly, and I have to speak of Michael Faraday. Many of you know something of the history of that very remarkable man. You know how he was born poor, how he was apprenticed when very young to a bookseller and bookbinder, how he had an insuperable love of science, and how he commenced to make experiments as soon as ever he could. While he was still at work at the bookbinder's he wanted very much to make an electrical machine. He knew something about such machines from an article on electricity he had read in the Encyclopædia Britannica, and so he went and spent 7*d.* in buying two bottles; but unfortunately his 7*d.* was thrown away, for he could make nothing of the bottles. He wanted to have a really good glass cylinder, and the one which he coveted was 4*s.* 6*d.* in price. He looked at it for a long while, and saved up his money, and when he had 2*s.* he borrowed the other half-crown from his master, and went off with the 4*s.* 6*d.* and triumphantly carried home this glass cylinder. You know probably that his father was a smith, and so his father set to work and forged this iron axle, and passed it through these corks and through the cylinder, and then the son Michael made what he called the first chemical experiment he ever performed, which was to dissolve some sealing-wax (in naphtha, I suppose.) He took this sealing-wax solution, and rubbed it all round here, so as to insulate the whole thing as well as possible, and then to cover over the blemishes of his workmanship he made it beautifully red with this vermilion all round the two ends. In the cellars of Mr. Riebau, his master, the bookseller, there was a broken mahogany table, and he got hold of those broken pieces and constructed this stand which you can see is of real mahogany,

and he got somebody else, (I believe he paid *1d.* or *2d.* for it) to make this additional support. Then his brother was also in the trade of a metal worker, and he made this very ingenious brass spring by which this rubber with the attached silk is caused to press hard upon the glass cylinder. It is very simple and very ingenious. Then he made this prime conductor, with a sort of fork with three points; and then this clamp was made so as to fix his apparatus to the table and hold it firm. But there were some portions which have been lost since. This conductor, of course, must have been insulated upon a glass support, instead of the wooden support which is put there merely for convenience, because the glass one is lost. There was also in all probability a second mahogany wheel corresponding with this one, with a band put round it in order that he might turn it well. Then there was the amalgam on the rubber, and by turning the cylinder he was able to get sparks, and all the common phenomena of electricity known at that time. I cannot show you any sparks from this machine: I have tried, but the fact is, it is rather old, and it is out of working order: the amalgam too is all oxidated; and it appears to me that to polish it and put it in order would be a kind of desecration of the old instrument, and thus I have not attempted it.

That machine was made and worked by him while he was still a bookbinder's apprentice, but he afterwards got an introduction to some of Davy's lectures at the Royal Institution. He read very fully Davy's writings; he became an enthusiastic admirer of him, and wrote out his last four lectures; and then he gave the book to Davy, and asked to be employed by him. I should like to go on with his whole history after Davy took him into his employment, and show you how this poor lad gradually worked his way up, by his industry, his perseverance, and his high character, to be a good lecturer and to be a first-rate experimenter; so that he became eventually, what I think we may call, the prince of these two departments. And although Davy had made the fame of the Royal Institution, Faraday sustained that fame during the many years he was connected with it. During

those fifty years he worked upon many chemical and physical subjects, and upon many electrical matters, opening out new regions of knowledge to us. The Fullerian Professorship of Chemistry was founded in order that he might be the professor. I wanted to include his work in my lecture to-day, although of course I could have occupied the time with Davy alone, because I knew Faraday well and loved and admired him so much; and when, between two and three years ago, the managers of the Royal Institution asked me to undertake the duties of the Fullerian Professorship of Chemistry, though I had a good deal else on my hands at the time, I could not decline so honourable a request, and I accepted the professorship. Therefore, as one who holds at the present time the chair which was founded for Faraday, I thought I must say something about Faraday. But my colleague, Professor Tyndall, the Professor of Natural Philosophy, has already brought before you last Saturday, in his able manner, many of the discoveries of Faraday, especially his electrical researches, and therefore I shall leave them entirely aside, and confine my remarks and illustrations to his chemical researches. I must mention one which he commenced when very young, and which he carried on at times almost throughout his life. Davy was experimenting with this very remarkable gas, chlorine. He obtained it combined with water, and he and Faraday, between them, made experiments and produced an oily liquid which Faraday found to be liquid chlorine—chlorine which was so much compressed inside a tube that it assumed the form of a liquid instead of a gas. This set Faraday working in that direction. You know in those days people thought there were certain things which were gases, like air, or the gas in mines, or oxygen, or nitrogen, or hydrogen. These were always gases, and nothing but gases, in their estimation. Then there were certain other things which were always liquids, such as water, or spirits of wine; you could boil them off into vapours no doubt, but still they were liquids. Then there were other things which were solids, such for instance as glass, which retained particular forms, and were affected in shape very little if at all by gravitation. A liquid will fall down and

assume the form of the vessel into which you pour it, with a flat surface on the top; and gas also you may pour into a vessel, and if heavier than air it will fall down like a liquid, but the gas will have a very uneven surface, and gradually it will rise up and fill the whole vessel, mixing with any other gas that is there; besides, a liquid is very slightly compressible, while a gas is extremely compressible and elastic. Now one of Faraday's works which he carried on throughout his researches, was the breaking down of boundaries. There was this boundary drawn between gases and liquids which could be vaporised; he showed that gases were merely what we may call permanent vapours; that if you can only heat a solid sufficiently without decomposing it, you can melt it into a liquid, and then you can raise it into a gas; and if you take the gases and compress them enough, or cool them enough, you can reduce them into a liquid and afterwards into a solid form, so that it would seem that every substance, which is not decomposed by heat or cold, is capable of existing in the three forms of gas, of liquid, and of solid. This is the pump which Faraday used for squeezing very many of the gases. These are the tubes which he employed, and there are many other things here, nuts, screws, spanners, and all sorts of instruments. Here are a number of the results in this box. Most of them were made in this way:—Faraday took the materials from which the substance is produced, placed them in a stout sealed tube, and thus prepared the gas under great pressure. He generally found that with sufficient pressure it became liquid. Of course his tubes frequently burst in the operation, but he took the best care he could to protect himself. And here are many of the products in these tubes, all of them no doubt under very considerable tension. I do not know how far you will be able to see them arranged at the bottom of the box from any distance; but there is liquefied chlorine, liquefied ammonia, liquefied laughing gas, or nitrous oxide, and there is liquefied muriatic acid, hydro-bromic acid, and many other things. Well, I cannot take these out and show you that the liquids would become gaseous if one removed the pressure, for one does not want to spoil the specimens. However, I will take laughing

gas, that nitrous oxide which Davy investigated so very fully, and which Faraday condensed into a liquid. It is now used largely by dentists for producing anæsthesia. Mr. Williams will let a little of it out of this strong iron vessel by gently turning the tap, and we will collect it over water. It is a compound of nitrogen and oxygen, with which many of you are quite familiar, because it is well-known in chemical experiments; but I will show you at any rate that we have a gas here in which a substance will burn better than in common air. You perceive that this piece of wood which was only red hot catches fire when put into this gas. Well, now, instead of allowing it to come out in the way in which it did just now, to pass through that water, I will cause it to come out into this little tube. Here is the liquid, as you perceive, at the bottom of the test tube, and there is a solid at the top which has been condensed, and I think you can see there is a good deal of it which has frozen on my handkerchief. Of course it is evaporating very rapidly; I do not know what enormous amount of temperature above its boiling-point it is exposed to, but it is slowly boiling away and rising into vapour. If we put a little piece of red-hot charcoal into the tube, it burns away very readily in the liquid nitrous gas. You see some solidified gas there, and I think we can produce it in a more satisfactory way still by causing the gas to come out intermixed with a quantity of air, when it will evaporate so rapidly as to produce such intense cold that it will freeze the liquid as it condenses, and cause it to fall down as snow. I am enabled therefore to show you this nitrous-oxide, which is ordinarily a gas, and which Faraday first produced in the liquid form, as a liquid, and also as a solid, which Faraday never saw. I should have liked to have exhibited to you some of the simplicity of his arrangements, but time is passing very rapidly. We saw the ingenious way in which, when quite a youth with no money to spare, he made this electrical machine. He carried out the same principles in after life. At a very advanced period of his career he made these different pieces of apparatus; he would work away with those ingenious fingers of his, twisting wires, cutting out cards, melting sealing-wax, and so on;

constructing the most ingenious contrivances, such as these on the table, in connection with his magneto-electric researches, many of which bear indications of the bookbinder's apprentice, but I cannot dwell upon that.

I should like to say just a word or two about some other qualities of the man. There was the power of his imagination by which he was able to see beyond what his contemporaries saw, so as to picture the various forces of nature, and figure to himself what might be taking place in the phenomena. But this power of imagination, though a quality of the very highest order, and perfectly indispensable for genius or for the higher achievements of scientific research, is, you know, a quality which requires curbing and holding in, but Faraday had also another quality, an intense love of truth, a determination that he would never rest until his mind was perfectly satisfied that he had got hold of a fact, and with those two qualities he was perfectly free to follow any theory. He started usually upon his researches without having made up his mind as to what was going to take place, but with his mind perfectly open and receptive to the truth of whatever nature might indicate. He worked away all day in his laboratory, usually among the kind of apparatus which we see here. That was generally his life,—working in the laboratory, or giving lectures in the theatre above; the prince of experimenters, the prince of lecturers. When the day's work was over he went upstairs into the bosom of his family; he was a man of much domestic feeling, and there with his wife and nieces he usually spent the evening, or he went out to divert himself with some of the various shows or amusements of this metropolis, the Zoological Gardens being a great favourite. On Sundays he always found his way down to the church of which he was an elder, and Wednesday afternoons he also devoted to his church. Frequently he visited the sick, whenever they required his solace and care. Beyond his laboratory, his home, and his church, he had but little life; he entered not at all into politics, and very little into any of the social questions of the day; but there was one question he did express his opinions on very strongly and worked a little at—and that was the subject of education. He was very desirous that the physical sciences should

have a much larger part in general education than they had in his day. I may perhaps read you his own words on this subject: "That the natural knowledge which has been given to the world in such abundance during the last fifty years should remain untouched, and that no sufficient attempt should be made to convey it to the young mind growing up and obtaining its first views of these things, is to me a matter so strange that I find it difficult to understand. Though I think I see the opposition breaking away, it is yet a very hard one to overcome. That it ought to be overcome I have not the least doubt in the world." Since his day, it has been largely overcome, and we find now the evidence round about us of the great interest that the people take in all matters of science, and we find that it is wending its way into our various schools: and so it ought. I am quite sure if Faraday had been standing in this place this evening, and had been enchanting you as he would have done with an account of his own experiments, he would very likely have spoken of the great educational advantage of this museum and of the adjoining buildings, and he would have claimed for natural science an honoured place in the education of every Englishman and Englishwoman.

Seeing we are in the midst of various forces which act upon us, and are surrounded by different kinds of matter, our bodies subject to the laws of chemistry, of physics, of heat, of electricity, and of mechanical force contending with them at every moment, it did seem to Faraday most important that we should understand them, and that we should not be their slaves but their masters: and that we can only become by studying them. Whatever other subjects must be learned (and we need not disparage those subjects), still I think it is the duty of those of us who may have any influence, to promote by all means education in these physical matters, in the knowledge of those forces and substances among which God has placed us, and in studying which we trace the workmanship of his hands. In this way we can enrich our minds, and find ourselves more fully furnished for the battle of life; and depend upon it, just as Davy or Faraday found that some of their researches, which seemed the farthest away from any practical application, in process of time were made

subservient to the use of man, so we shall find now. There are certain minds, such as the minds of the two men I have brought before you, especially the latter of the two, which seem to work in the outermost regions of knowledge; they do not think of how their knowledge is to be brought down to the common needs of humanity; and yet we know that both Davy and Faraday were far from indifferent to what might benefit their fellow-men. We have seen one instance in the enthusiastic way in which Davy set to work and entered into those experiments on flame which I have described so briefly. It is almost inconceivable to us how he could have done so much in so short a time for the benefit of the men working in the mines. And similarly Faraday sometimes took up subjects which bore upon the necessities of life, and at other times his most recondite discoveries bore practical fruit even in his own lifetime.

I should have liked to have said something to you about his magneto-electrical experiments, but I must pass on. I have already shown you Davy's note-book, and here is Faraday's note-book in which he describes his experiments with nitrous oxide. Here also are some of the large coils, natural magnets, and so on, with which he first obtained the evidence that from magnets we can get a spark of electricity. As he first produced it, it was a little spark, so small that it could hardly be seen, and it would be impossible even in the dark to show it to such an audience as this; but Faraday, even in his own days, had the satisfaction of seeing his little baby grow up into a giant; he saw the little spark which he had been the first to produce exalted by others so as to produce one of the most brilliant lights we have. Davy himself showed that if we take the Voltaic battery, or any other source of Electric power, and cause a current to pass between charcoal points, we get a brilliant light. Now if we produce Faraday's magneto-electric light between charcoal points we get a bright light too. It was Mr. Holmes who suggested this for lighthouse purposes, and Faraday, being at that time the scientific adviser of the Trinity House, was called upon to examine it. I at the same time happened to be serving on the Royal Commission on Lighthouses, and thus I had the happiness of being associated with those who were working on

this electric light, and of making some of these experiments with Faraday. Faraday worked away at this matter until he convinced himself that this light was practically available for the purpose of lighting our shores. It was put up at the South Foreland, and afterwards removed to Dungeness Lighthouse, where I dare say you have seen the brightest spark on earth shining from the lighthouse. Thus has Faraday's little magneto-electric spark been exalted into that marvellous blaze. If you go into the Loan Collection, in the part devoted to Electricity, you may see shining a light which is produced by the Alliance French Company, where a number of coils are caused to revolve round fixed magnets so as to produce electric currents, and these currents passing between charcoal points give an intensely brilliant spark of light. This spark is used in some of the French Lighthouses, at Cap La Haye, for instance, and I believe at some other parts of the coast also. This then is one of the applications of Faraday's discoveries.

But time forbids that I should go further into these matters. I have endeavoured to illustrate the fact that there are two modes of life possible to such a man as the one we are now considering: there is the purely intellectual life which the philosopher may lead in his study, and his laboratory, and that beautiful life which Michael Faraday did actually live, a life full of love for all his fellow-creatures, attaching everybody to him, working quietly, unostentatiously, perseveringly, living an honest godly life, and at the same time producing those marvellous results that won for him distinction all over Europe, and started a great number of lines of thought which have since produced some of the most wonderful discoveries and inventions that have ever benefited mankind.

The CHAIRMAN: Ladies and Gentlemen,—I have the honour in the name of the Lords of the Committee to thank Dr. Gladstone for his most interesting lecture, and I am sure you will join with me in those thanks to the Lecturer who has given us so very charming and instructive an hour and twenty minutes. I shall be happy to report to the Lords of the Committee that we have been honoured here with the presence of a very large and most attentive audience.

ASTRONOMICAL INSTRUMENTS.

By The Rev. R. Main, F.R.S.

July 10th, 1876.

Professor Guthrie in the Chair.

The Chairman: Ladies and Gentlemen,—It is the custom that the Lecturer should be introduced to you, and therefore I go through the form of introducing to you a gentleman who is infinitely better known, to you and to the world of science than I am,—the Rev. R. Main, the Director of the Radcliffe Observatory, at Oxford.

The Rev. R. Main: The brilliant discoveries in the Physics of Astronomy which have been recently so prolific have perhaps thrown rather too much into the shade that older, and possibly on the whole more important, portion of the whole science called Practical Astronomy.

In both departments the workers and discoverers have been engaged with equal industry in disentangling the phenomena derived from laborious and constantly repeated observations and experiments; in establishing on a firm basis theories derived from the facts; and in finally giving to the world the results of their labours in a form adapted for popular apprehension.

In both departments there have been applied the same cautious induction of particulars, the same tests for the development of the truth, the same industry, and the same sagacity; and the difference in the popularity of the two, is due rather to the rapid and brilliant success which has been the lot of the physical inquirers, compared with the slow growth of Practical Astronomy, which has required centuries of continuous observations and

severe mathematical labours before the great results could be presented in a shape to attract the public.

I have thought therefore that I should be doing some little service by attempting to show in as untechnical a way as the subject admits of, the nature of the instruments and the observations by which the great triumphs of Practical Astronomy, as distinct from the physics of astronomy, have been attained; the subject is of course more peculiarly connected with my own office, and I can handle it with greater confidence; and all the apology I need make is for the necessarily dry details of certain portions of the subject, which will perhaps tax your patience rather more than I could desire.

As the subject is an extensive one, it will not be worth while to consider in any great detail the instruments and methods used by the ancients, nor even those used throughout Europe in the last century, as all the very great improvements which have made practical astronomy so exact a science have taken place in the present century. But for persons unfamiliar with anything but the results of observations in their popular shape, it may be desirable to state what is chiefly required in every method of observation, and how the instruments themselves are limited, and, we may even say, in their essential features, designed, by the phenomena of the heavens.

Every one is familiar with the fact of the earth's rotation, uniformly from West to East in a definite space of time, which is used as the unit of time for civil and astronomical purposes. The axis on which it turns, which, for our present purpose, we may consider to be always parallel to itself at all seasons of the year, will meet when produced the sphere of the heavens at two points, which though not absolutely fixed, are absolutely determinable, and must be found by observation. These two points, or poles, then, are the fundamentals in practical astronomy; they are the fixed (used in the former sense) points of reference; and from them we obtain immediately a *plane* of reference, namely, the equator, or great circle which passes through the earth's centre at right angles to the axis.

Now the great work of practical astronomy is to determine

for celestial objects at unlimited distances, such as the stars, their angular distances from certain planes of reference, and, for the planets, and other objects whose linear distances from the earth are finite, not only these angular distances at different times, but also their linear distances from the earth, and the orbits or paths in which they move.

The plane of the equator, therefore, naturally suggests itself as a convenient plane of reference; and if, by any means, instruments can be constructed which will determine the angular distance of each object whose position is required, north or south of this plane, we shall by this means have determined one element of its position.

Now, naturally the most direct and convenient instrument for this determination is what is called the *Equatorial*, which in its simplest shape consists of a telescope attached either immediately or intermediately to a rigid axis, which is paralled to the earth's axis of rotation, and with circles attached on which can be read off the angular distance of the observed object from the equator, and also the angle made by the plane of a great circle passing through the poles and the object, with the plane of the meridian or second fundamental plane of reference.

The meridian plane is, in fact, the vertical plane, corresponding to the observer's position on the earth's surface, which passes through the zenith (or vertical point) and the poles.

And the mention of the plane of the meridian (which has not been previously defined), will immediately lead to other considerations, which will tend to show that the equatorial, admirable and useful as it has proved to be for differential measures, is not capable of being used for fundamental determinations requiring absolute accuracy, but that others, named meridian instruments, are of necessity employed for this purpose.

In defining the meridian plane, we had occasion to mention the zenith, or point of the heavens immediately vertical at the place of observation, and this evidently introduces the consideration of gravity. The zenithal line is, in fact, the line perpendicular to the plane of standing water or of unruffled mercury, or it may be defined to be that which is coincident with the direction of the plumb-line, that is, of a line stretched by a weight and hanging freely.

The principle of the horizontality of the surface of fluids under the influence of gravity, is made use of in two ways, namely—

1st. By the use of what is called the spirit-level, which consists of a glass tube of very small curvature very nearly filled with fluid, but having a void space or bubble in the centre, and furnished with a divided scale for noting the changes of the position of the two ends of the bubble, corresponding to changes in the level of the line or plane whose inclination is required:

And 2ndly, by the use of mercury, by which the reflected image as well as the direct image of an object viewed through a telescope may be observed.

The use of plumb-lines has been in a great measure discarded excepting in the case of certain zenith sectors of not very modern construction.

We will now endeavour to show how, by observations made in the plane of the meridian, the use of a surface of mercury enables us to determine that one element of position of a heavenly body before alluded to under the name of Polar distance or declination. To do, this a reference to a figure will be necessary.

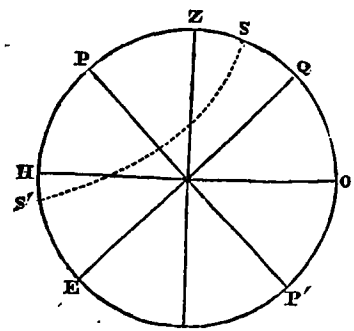


Fig. 1.

Let H P O P', Fig. 1, represent a meridional section of the sphere of the heavens, passing through (as previously explained) the poles P and P', and the zenith Z. Then the circle E Q, seen in section, at right angles to P P', will be the equator; and finally let S S', denoted by a dotted circle, represent the semi-diurnal course of a star or other object, from the upper meridian at S to the lower meridian beneath the pole at S'.

Then, since Z is the zenith of the place of observation, the earth being supposed spherical, Z Q will represent its latitude, and this will plainly be equal to P H, or the altitude of the pole at the place—thence we get this important fundamental rule that:

The altitude of the pole is equal to the latitude of the place of observation.

Also, P Z is the complement of H P, that is 90°—H P, and is therefore the co-latitude of the station.

P Z then will, for this place, be a constant which must be very accurately determined, and one part of our duty will be to show how this may be done.

But, premising that it has been done, and also assuming that the zenith distance of an object S can be determined, then it is plain that P Z + Z S, or the constant co-latitude, added to the zenith distance of the object, will give P S the Polar distance, or distance from the pole.

What is required then in fixed observatories, as one of the fundamental classes of observation, is to determine with all attainable accuracy the zenith distances of all objects, and we will proceed to explain the means by which this has been effected. Though apparently a simple problem, the perfection of the means for accomplishing the task was not attained except by slow degrees and with a great deal of trouble.

During the last century, and indeed from the time of Bradley, mural quadrants were invariably used, though nothing could have been worse in principle or more clumsy or inconvenient in practice.

Imagine a quadrantal arc of 8 feet radius, strongly braced, and furnished with a telescope rotating on an upper pivot in the plane of the quadrant, firmly attached to the eastern wall of an immense pier in the middle of an observing-room, and pointing to south stars, and a similar one on the western pier pointing to north stars. These were generally of excellent workmanship, and Graham and Bird, the most celebrated makers of the day, employed all their skill on those erected at Greenwich and Oxford. Yet in themselves they were utterly helpless, and incapable of determining their zero-points or of eliminating by any method of observations

the errors of torsion, flexure, or division, to which they were, from their weight and unsymmetrical arrangement, peculiarly liable.

For the determination of zenith point or zero of zenith distance, a subsidiary instrument, namely, a zenith sector, was employed.

This instrument (of the class of those made memorable by Bradley's discovery of the aberration of light and of nutation), was reversible round a vertical axis, and was furnished with a plumb-line for observing the defect of verticality of the tube, so that by its means the absolute apparent zenith-distances of certain stars passing the meridian of the place of observation near the zenith could be determined with tolerable accuracy.

Nothing could be worse than the whole arrangement, and yet Bradley by his unwearied diligence and exquisite skill as an observer managed to make, with such instruments, observations which resulted in a catalogue of more than 3000 stars, which, when discussed by the celebrated Bessel are proved to be but little inferior to the best modern observations, and the great work including this catalogue is justly named by him (Bessel) the *Fundamenta* of Astronomy.

The Astronomer Maskelyne, during the whole of his long career at Greenwich, used the quadrants, though it is but just to state that he had, some time before his death in 1811, recognized their defects and errors, and had given the requisite directions to the celebrated Troughton for the construction of a mural circle. This circle was not completed and established at Greenwich till the year 1812, at which time Mr. Pond had become Astronomer Royal. Its introduction was probably the greatest revolution and improvement that was ever effected, and it was due in a great measure to the genius and industry of the astronomer who used it afterwards with so much effect: Mr. Pond had, several years before, with a small circle which he used at Westbury, drawn attention to the errors of the Greenwich quadrants, and had given, in the Philosophical Transactions for 1806, a full discussion of his observations. He was probably, therefore, the only man in England at that time capable of bringing to perfection the use of the new circle, of encountering the difficulties of its management and adjustments, and

of meeting successfully the opposition which its introduction raised amongst astronomers, who had been habituated to the use of the quadrants.

Mr. Pond's mode of use of it (which continued during the whole time of his tenure of office), was singular and ingenious, though there is a trace of that unnecessary complexity in the solution of a problem which to us appears so easy, which, I suppose, is to be observed in the successive advances of all the sciences. In 1825 another circle, which it was intended should be furnished to the observatory at the Cape, was sent to Greenwich to be examined and verified before its departure, and Mr. Pond conceived a scheme for the determination of the zenith point by the use of the two circles, which induced him to ask the Government for permission to retain it. This was granted, and, up to the time of the commencement of the present Astronomer Royal's directorship, both continued in use.

Mention has been made of the various ways by which the zero of zenith distance can be determined, but it may be now mentioned that, on the introduction of the mural circle, mercury alone was henceforward employed.

Imagine a trough of mercury with an unruffled surface placed beneath the object glass of the telescope in such a position as to reflect the rays coming from a star up the tube. The reflected image of the star will then, by proper arrangements, be observed, as the star itself would have been by direct vision; or, a reading of the circle will be obtained for a point of the meridian whose angular distance below the horizon is precisely equal to that of the star itself above the horizon. If now by any means the reading of the circle for the star observed by direct vision could be obtained, the half sum of the two readings would plainly be the reading for the horizon or the horizontal point, and, if 90° be substracted, we get the reading for the zenith, or the zenith point. It was the practice then, under Mr. Pond's direction, to observe on one evening a certain number of stars by direct vision with one instrument and by reflexion with the other, and, on the following evening, to observe the same stars directly with both instruments, so as to get very accurately the difference of the readings of the two instru-

ments for the same zenith distance, or for the same object. If this difference, thus found, be applied, therefore, to the direct readings of the circle which did not observe at all by reflexion, we shall reduce them to the direct readings which would have been found for those objects which were observed by reflexion with the other circle. Hence the zenith point would be found immediately for both circles by the method which I have previously explained. Now every one here would probably be inclined to say (what is perfectly true) that this use of two circles though very ingenious was very troublesome and complex, and left great room for improvement.

And the improvement came from the source from which it ought naturally to have come, namely, from the illustrious successor to Mr. Pond at Greenwich, the present Astronomer Royal, while he was director of the Observatory at Cambridge. He saw clearly that there was no real difficulty in making the double observation (that is by direct vision, and also by reflexion) at the same transit of a star with a single instrument.

The only difference would be, that the reflexion-observation must be made at one of the side wires, preceding the transit across the meridian, and the direct observation about a minute afterwards, at one of the other side wires after the transit. And this would involve trifling corrections for the curvature of the apparent path of the star across the field, which, excepting at the equator, would not be a great circle.

This method, then, devised by Sir G. B. Airy, has been generally pursued ever since his assuming the directorship of Greenwich, though, as the observation of an adequate number of stars by reflexion is laborious, I am not certain whether at some of the continental observatories it is pursued with so much rigour as it ought to be.

From my own practice, I feel convinced that observations of stars by reflexion are generally indispensable, and that, if the zenith point be obtained (as is done in some instances) solely by observation of the reflected image of the wire, certain errors of considerable importance might escape notice.

With respect to this I ought to explain that "the reflected

image of the wire" has reference to an ingenious way of making the telescope its own collimator; that is of seeing through it the reflected image of the wire as well as the wire itself, by means of an eyepiece called from its inventors "Bohnenberger's Eyepiece." Placing, by means of the declination-micrometer, these images in contact with each other and reading off, one gets a reading of the circle, in fact, for the vertical line or for the nadir; and, of course, by subtracting 180° from this, there is obtained the reading for the zenith, or the zenith-point.

If the circle were perfect, if it had no flexure, if we were quite certain that one part of it would give just the same results as another part, this method would be perfect; but that is not so. In my own case, I was puzzled for some time after taking possession of the Carrington transit circle, formerly used with so much effect by Mr. Carrington, and which is made, I may say, exactly after the model of the great transit circle at Greenwich;—I was puzzled, I say, by finding that there was a constant difference between the seconds of the zenith point, as got by the reflected image of the wire, and that obtained by stars. It was not badness of observation, nor anything which could be eliminated; it was as nearly possible 1½″, and it has continued ever since. At first it gave me some trouble until I studied the theory of it, and then I came to the conclusion that the circle had been made not quite solid and firm enough and that there was a slight quantity of flexure; and any flexure in the circle will introduce a term for correction dependent on the cosine of the zenith distance. Assuming that I have been able to eliminate this correction, I believe the observations reduced by me are on the whole as accurate as those made at most other observatories; and for that reason I have taken great pains with such nice points as that which is called the R. D. correction, that is, the difference which exists after all pains taken between the final results of the reflexion and the direct observations. They ought to agree, but they never will agree in any final catalogue. But by means of applying a correction at an earlier stage of the work, I have been able to get rid of the discrepancy almost entirely, and there is no difference worth taking into account in the

results of the observations at the end. The means of the direct observations agree admirably well with the means of the reflexion observations.

Mention of the last great improvement which has been made more recently, by the substitution of the transit circle for the mural-circle, must be deferred till an account has been given of the construction and use of the other fundamental instrument for getting the elements of the position of an object. But by reference again to Fig. 1 it will be seen now, we hope quite distinctly, how the meridional zenith distances, and ultimately the North Polar distances, are obtained at fixed observatories.

Referring to the figure, it will be easily understood that the reading of the circle for an object in the zenith (that is, the zenith point), can be obtained by a sufficient number of suitably made reflexion-observations and direct observations at the same transit, that is, the zero point or index correction for the zenith, can be found with all desirable accuracy. Hence, for an object S on the meridian, its zenith distance will be immediately given by the subtraction of the zenith point from the circle reading; and, in a similar manner, the apparent zenith distances of all other objects will be found. These, of course, all require correction for refraction, that is, for the bending of the rays of light downwards in their passage through the layers of our atmosphere, and at this stage come in theory and calculation. The instrument has done its work, in giving the apparent zenith distances, and all the rest is labour for the computing room.

But, assuming the corrections for refraction, into which subject we cannot enter now, it will be easy to show how the co-latitude, or arc Z P, peculiar to each separate observatory, is obtained fundamentally. Of course, if a provisional result only were wanted, it would be easily obtained by comparing the true zenith distances deduced from observation with the tabular N. P. D's of certain of the fundamental stars, which are given in the Nautical Almanac with almost absolute accuracy.

For instance, the arc P Z which is the co-latitude, is the difference between P S the North Polar distance, and Z S the zenith distance, and, if the object at S be one of the fundamental

stars, as Capella, α Orionis, or α Aquilæ, whose tabular places can be scarcely improved, the co-latitude will be obtained immediately by the comparison.

But every observatory is required to determine its latitude and also its longitude fundamentally by means of its own observations, just as if no previous observations existed of any of the stars employed.

An easy means of determining the co-latitude is by observations of the polar star (Polaris), or some other circumpolar star near the pole, at its upper and also at its lower transit. Thus, let S S′ be the small circle described by Polaris, coming to the upper meridian above the pole at S and to the lower meridian at S′ Then it is plain that the apparent zenith distances Z S and Z S′, can be determined by the methods before explained, and, after they have been corrected for refraction, their half sum will be Z P, or the colatitude, because, as the star describes a small circle round the pole, P S will be equal to P S′. By repeating, therefore, observations of this character at every favourable opportunity, and taking care that there shall be tolerable equality between the number of observations above and below the pole, a very accurate result can be obtained for the colatitude in one single year. It should at the same time be noted that the colatitude depends, when we come to minute accuracy, on the constant of refraction used in the reduction of the observations; and, as an instance, we may refer to the recently published volumes of the Greenwich Observations, where it will be seen that a very small change in the refractions employed has been followed by a perceptible apparent change in the co-latitude.

Enough has probably now been said to make any intelligent person, without further knowledge of astronomy than that which is given in the course of an ordinary education, to understand how, in a fixed observatory, one element of the position of an object, namely, its north polar distance (when referred to the pole), or its declination (when referred to the equator) is obtained.

But this is not sufficient. To determine the position of a point on a plane surface, *two* co-ordinates, as they are called, are required. Two lines of known position (usually at right angles

to each other) are taken, and the position of the point is defined with respect to these lines, which are called the axes of co-ordinates. Let A O B and C O D (Fig. 2) represent these lines or axes, then, if we know the distance of the point P from each of these axes, it is plain that we could find or lay down its position by measuring off along the axes those distances, and drawing parallels to them, whose point of intersection would plainly be the position of the point required.

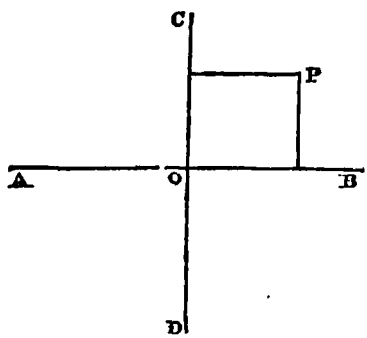


Fig. 2.

In geography and astronomy the problem is of a similar nature. In geography, for example, the position of a place on the surface of the earth, assumed spherical, is defined by its latitude, or angular distance above or below the equator, and by its longitude, or its angular distance along the equator, measured from an arbitrary fixed point.

Similar elements are used in astronomy; the declination of an object evidently corresponds to terrestrial latitude, if we substitute the sphere of the heavens for the spherical surface of the earth, and the other element (called right ascension) similarly denotes the angular distance of the object from a determinable fixed point on the equator.

Thus, in Figure 3, E M is a projection of the plane of the equator on the celestial sphere, S the position of an object referred to the equator by the arc of great circle S M, perpendicular to E M. Then S M is the declination and E M, measured from some point is the right ascension; and we have now to consider

how E M is to be determined by observations, and what point must be taken as the origin of co-ordinates.

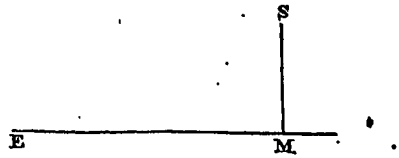


Fig. 3.

Of course it is necessary that the point E from which we measure should be determinable by observation, and that it should be in common use by all astronomers; otherwise catalogues of the places of stars, or right ascensions of other objects, would not be easily comparable with each other.

Now such a point happily exists. I dare say all of you know that the earth moves in a plane orbit round the sun, or that the sun apparently moves in a plane orbit round the earth, called the ecliptic. And as the earth is taken as the centre of our visible sphere, the sun will appear to describe a great circle in the heavens, cutting the projection of the equator on the sphere of the heavens at two points diametrally opposite to each other, called the vernal and autumnal equinoxes. The point of intersection corresponding to the vernal equinox is taken as the point from which we measure right ascensions. If this point were absolutely fixed, astonomers would be spared a good deal of labour; but it is not so.

It moves backwards on the plane of the ecliptic, on account of the disturbances produced by the sun and moon on the protuberant matter of the earth's equator, by about 50″ in a year. As, however, its motion is accurately known by theory, the point is supposed to be actually fixed, and the calculated corrections are applied to the places of the stars in the opposite direction.

The great advantage of the equinox as a point of reference is that its position depends on the sun's apparent motion, and can be determined with very great accuracy.

It will be necessary to explain how this is done, but it will be better first to describe the instrument with which right ascensions are observed, that is, the transit instrument.

The principle involved in this instrument may be understood from very simple considerations.

Imagine the meridian, that is, the vertical plane passing through the poles, or the north and south vertical plane, to be represented by something material, such as a wall, and imagine great circles of the sphere, meridians, in fact, to be drawn through the poles and to pass across certain stars whose right ascensions are required, and therefore cutting the equator at corresponding points. Then by means of the earth's uniform rotation, these stars would evidently be brought into the plane of the meridian, at intervals of time proportional to the portions of the equator intersected by the great circles corresponding to them; and if a person were to watch the time at which any one of the stars appeared to be in the meridian plane, or to be eclipsed by the wall representing it, and adjust his watch so that its index should pass over exactly twenty-four hours between this transit of the star and that on the next day, then the differences of the watch-times for the transits of the other stars would give the differences of their right ascensions, and the watch would be adjusted to what is called sidereal time.

This of course would be a very rough kind of observation, but the transit instrument really enables us to make the same kind of observation in a more refined and accurate way.

Instead of the material plane which I have supposed, let us imagine a telescope whose optical axis or line of sight should by revolution round an axis at right angles to it always describe or sweep out this plane which I have imagined. It is clear that the same would hold good, and that the watch-times at which stars would arrive at the wire or mark in its field of view which indicates the line of vision, would give the differences of the right ascensions of the stars; and the conditions of the problem will immediately tell us in what way this telescope is to be mounted, and what is the nature of the adjustments required.

For first, it is, by revolving round an axis, to enable its line of sight to describe a vertical plane; and therefore this axis, that is, the imaginary line joining the two pivots on which it turns, must be horizontal.

Secondly, it is to describe *that* vertical plane which is the meri-

dian, or north and south plane, and therefore the direction of its axis must be exactly east and west.

And, finally, its line of vision, or the direction of collimation, as it is called, must describe a great circle, and, therefore, this line must be at right angles to the axis of rotation.

These then are the three adjustments required; the first for level of the axis; the second for azimuth; and the third for collimation; though the last, namely, that for collimation, which is merely mechanical, is generaly considered first.

It will not be necessary to go into any great detail to explain to you how these adjustments are performed, as I am aiming in the whole of this lecture to explain only the principles on which the whole science of practical astronomy rests, and these are best seen where there is but little complexity to distract the attention.

Still it is necessary to say a little more about the general construction of the transit instrument and the way in which it is fitted up, that every one who is not either a professional or an amateur astronomer may have distinct notions about it.

First, then the telescope (whose tube is made very stiff and strong, especially at the centre where it has to carry the axis of rotation with its cylindrically ground and polished pivots), is supported on two massive stone piers built up from a foundation of hard gravel, or, in defect of that, a bed of concrete laid on the natural foundation, and the pivots themselves are held in what are called brass Y's, from their resemblance in shape to that letter.

Then across the tube of the telescope, in its principal focus, there is placed a metallic frame, across which are stretched a series of very fine material lines (technically called wires) composed of spider's web. Of these one (or two sometimes) is placed horizontal to mark the part of the field at which the star should be placed for observation, and all the rest as nearly as possible vertical, as over these the times of transit of the star are to be observed.

The number of transit-wires, placed as nearly as possible at equal distances, is usually five or seven, but always an odd number, of which the centre wire is intended to mark approximately the line

of collimation, that is, the line which is to be in all positions of the telescope at right angles to the axis of rotation; and transits are generally taken over all the wires to secure greater accuracy, and the mean taken, the difference between the transit over the mean of wires and over the central wire being always small and easily calculable.

The first adjustment or correction required is for the position of the central wire, or, as it is technically called, for the error of collimation. This wire should be so placed that the vertical plane passing through the optical centre of the object glass at right angles to the axis of rotation, should pass through it. If it does not, the line of collimation, that is the line joining a point in this wire and the optical centre will describe a *small* circle either to the east or west of its required position. It is usual, for the purpose of measuring this deviation, to attach a micrometer to the eyepiece with a wire moving parallel to the transit wires and in their plane, and to observe in reversed positions of the axis of rotation either a distant mark, or the cross in the focus of a collimating telescope which is visible in the field of view of the transit telescope. If the wire require no correction the angular distance of the object observed from it will be the same in both positions; but if, as is generally the case, the distances are not the same, half the difference will give the value of the error of collimation, and a numerical correction, easily computed, is applied to all the transits, to take account of the error.

The line of collimation or line of sight of the telescope now describes a great circle in the heavens, but not necessarily a vertical circle. That this condition should be fulfilled, it is necessary that the line joining the centres of the pivots should be horizontal, and that the pivots should be perfectly cylindrical and perfectly equal. To test this, an apparatus is applied called the spirit-level. It would occupy us too long to describe this instrument minutely, and perhaps enough has been said in a former part of this lecture to explain the principle of its construction. It may be said, however, that levels are of two kinds, striding and hanging levels, the first being placed above the axis, with Y's adapted for attachment to the pivots, and the second being below with the Y's hanging on the pivots above.

The error of level, found with this instrument, being corrected, or allowed for numerically, the line of collimation of the telescope will now pass through the zenith, that is, it will describe a vertical great circle, cutting the meridian at the zenith but not necessarily coinciding with it. That this should be the case it is necessary that the horizontal line joining the centres of the pivots should be exactly east and west. To determine the error arising from want of coincidence with the meridian, or the azimuthal error as it is called, recourse must be had to observations of circumpolar stars, Polaris, as every one knows, being the most important.

Imagine that the transit of Polaris, across the central wire, can be observed at both the upper and the lower passage, that is, both above and below the pole. Then if the central wire coincide with the meridian (no collimation or level error existing), the interval in time both between the upper and the lower transit, and between the succeeding lower and upper transit, ought to be exactly twelve hours. If, as is usually the case, the intervals should be unequal, the north side of the plane of collimation deviates to the east or west of the meridian;—to the west, if the interval from upper to lower transit be smaller than the succeeding interval, and *vice versâ*. The difference, however, gives means for calculating the numerical amount of the errors. Double transit of Polaris, or of other circumpolar stars (which latter are really and with difficulty obtained, as one transit must occur in the daylight) are exceedingly valuable, because they not only determine the position of the instrument fundamentally, but also the right ascension of Polaris, or other stars observed. But in defect of this, it is usual to employ another circumpolar star, differing from the first nearly 12^h in right ascension, whose position is well known, and to compare the right ascensions with the times of transit, by which means the error of azimuth can be determined. I will not spend more time on the construction and use of the transit instrument, as it would not be of any practical value to persons familiar with it, who may happen to hear me, and for others I wish only that the general principle of its mode of use should be understood.

Let this only be distinctly kept in mind, that there is used in connexion with it a clock, whose hour hand completes its entire

circuit, that is, twenty-four hours, exactly in the interval of time during which a star after crossing the meridian returns to it again; and that the zero of computation of sidereal time is the instant of transit of the point of the vernal equinox, or, as it is called, of the First Point of Aries.

The most interesting consideration for us now, will be of the means for the determination of the position of this point.

Imagine ourselves to be in the situation of Maskelyne, the Astronomer Royal, who had every thing pretty nearly to determine for himself. There were no catalogues of the places of fundamental stars to refer to, no Table of Corrections for the various inequalities in the apparent motions of the stars; he had simply the book of the Heavens spread open before him, and a transit instrument (not too good) and a mural quadrant, wherewith to read it or observe it.

As the position of the invisible but determinable zero of right ascensions (namely, the First Point of Aries), had still to be found, it was necessary to choose provisionally some other point of reference, and, of course, a fixed star easily suggested itself. α Aquilæ was the star chosen by Maskelyne, and his Ledgers of R. A. of the stars observed by him (which I edited while at Greenwich) contain for the first year, 1765, the right ascensions of all the rest compared with this star, that is, they were simply differences of R. A. Afterwards he used a provisional R. A. of α Aquilæ deduced from the observations of 1765. Then, for the determination of the equinox or zero of right ascension, as this plainly depends on the solar motion, observations of the sun both in R. A. and N. P. D. were made, especially about the time of the two equinoxes, as also at the two solstices for determination of the obliquity of the ecliptic.

Imagine now that the sun was observed in both elements on the day preceding and the day following the autumnal equinox; then on the preceding day his declination would be at noon a few minutes north, and on the following day a few minutes south of the equator. The figure No. 4 will represent this state of things, when S and S′ represent the successive positions of the sun in the ecliptic, referred to the equator at M and M′

by the perpendicular arcs S M and S′ M′; hence the arc M M′ will represent in arc the observed difference of R. A. corresponding to the observed declinations S M, S′ M′. In the small triangles S Υ M and S′ Υ M′ considered as plane, these are given quantities, and the obliquity or angle S Υ S′ is also known, having been determined by solstitial observations continued from the times of the ancient astronomers.

Hence it would be easy to compute the value of the arcs Υ M and Υ M′, the former of which subtracted from 180° is the sun's R. A. in arc, on the preceding day, and the latter added to 180° is the R. A. on the day following.

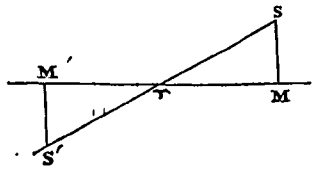


Fig. 4.

Comparing this then with the assumed R. A. of the sun depending on the assumed R. A. of α Aquilæ, we obtain the error of the assumption; and this, when applied as a correction to all the stars, would give their apparent R. A. referred to the apparent equinox of the day of observation.

This process was repeated at the vernal equinox, and thence a curious fact became evident, namely, that the R. A. of stars determined at the two equinoxes never agreed accurately. The reason of this at once became evident, and it depended chiefly on the defects of the tables of refraction which were employed in reducing the N. P. D observations of the sun. In the preceding portion of this lecture enough has been said to make this intelligible. The element which is observed is the *apparent* zenith distance of the object, and this has to be reduced to *true* zenith distance by an assumed value of the constant of refraction, and an assumed theory of the law of refraction. And this same difficulty occurs in the reduction of the observation of circumpolar stars employed for determining the latitude, which itself depends therefore on the refractions. But the method I am describing corrects the equinox not only for the effects of erroneous refraction, but for

that of the assumed obliquity, and instrumental errors as well Imagine for instance that the refractions, and consequently the sun's observed N.P.D. were too small, and let Σ and Σ′ be the true positions at the autumnal equinox. Here it is plain that the equinox will be pushed backwards to Υ′ and the computed arc Υ M′ will be too small, and therefore the R. A. will be too small.

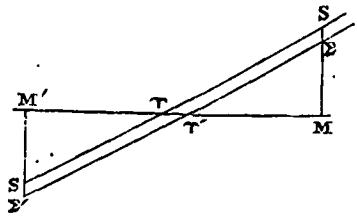


Fig. 5.

But at the vernal equinox, when the sun is ascending instead of descending, the arc Υ M′ will plainly be by the same quantity too large, and consequently the mean of the errors of assumed R. A observed at the two equinoxes will give the value required independently of the error of the refraction-tables.

This was the method pursued by Maskelyne in the formation of a fundamental catalogue of standard stars, but, of course, in more recent times, more scientific methods have been used (all however being based on the same principle), by which all the observations of the sun made in the course of the year can be made available. By this means having obtained a correction for the assumed zero or, to speak in mathematical language, origin of co-ordinates for right ascension, the apparent places of all the stars observed are obtained by means of their times of transit across the wires of the transit instrument, presuming that the clock keeps accurate sidereal time, that is, that it points to oh. om. os. when the *First Point of Aries* passes the meridian, and that it goes through exactly 24h. in the interval between two successive transits of the same stars; in fact, that it keeps exact sidereal time. But it is not likely to do this;—it will have in general an *error* and a *rate;* and this error and rate must be obtained by comparing the times of transit of such standard stars as have had their R. A.'s accurately determined, with their computed right ascensions for the year.

Maskelyne selected 36 stars for observation, and their computed

or tabular places could be used for deduction of clock rate and error, and then for the determination of the apparent right ascensions of all other objects which were observed, and by this process the number of stars accurately observed was constantly increasing.

On looking at the old nautical almanacs, I find that there was no catalogue of stars whatever put into them, which astronomers could use, until the year 1821, and then I think it was a catalogue of 20 stars, that became afterwards a catalogue of 60, and then 100, and this number continued until a late period. This shows how very slow the progress has been, and that notwithstanding these methods appear very simple and plain, it took a long time before astronomers were furnished with this basis for their future work.

But the method had, and still has, this disadvantage, that any error which may remain in the assumed R. A.'s of the fundamental stars, will be communicated to those of all the stars reduced by their means, and thus the determination of absolute right ascension is a slow process, and has exercised the abilities of the most eminent astronomers. It would occupy too much time and be beyond the scope of this lecture, to show how the daily apparent places of the stars obtained by the process above explained, are cleared of the inequalities arising from precession, nutation, and aberration, and their mean places (that is, their places cleared of these inequalities) are reduced to a given epoch, usually the beginning of each year of observation; and finally, how these yearly catalogues, which are published, for example, with with great punctuality at Greenwich and the Radcliffe Observatory, at Oxford, are finally combined so as to form catalogues for a mean epoch containing accurate places of several thousand stars. It must be sufficient at present to have indicated the general processes by which this is done; and it may stimulate my intelligent hearers to endeavour to obtain fuller information.

Thus far I have spoken of the mural circle and the transit instrument as distinct instruments, of course requiring separate observers. And indeed till a comparatively recent date they were so used; for example, Troughton's mural circle and the corresponding transit instrument were in use till the year 1851. Then

was established by Sir George Airy, the great transit circle, a work of great labour in the designs for its construction (which were made by himself), and which, as far as I can judge, is still unrivalled both for accuracy of principle and convenience in the daily use.

The transit circle then, as its name implies, is intended to combine in one instrument both the mural circle and the transit instrument, and this is effected by simply attaching to the horizontal axis of the latter (made of course proportionally strong) two circles, one of which is accurately divided for observations of zenith distance, and the other more coarsely divided to serve as a setting and a clamping circle.

The idea is so simple, that one is tempted to wonder, as we have seen occasion to wonder before, why the inconvenience of two separate instruments was put up with so long. One reason may be that all the instruments which we have been occupied in describing, are very costly and elaborate, and are usually employed in official establishments such as that of Greenwich, and that a serious responsibility is incurred in any organic change such as the replacing of such instruments by new ones. I believe that at the present time, such scruples would not exist in the same degree, and, that in any department of science, instruments of plainly defective principles of construction would be discarded. But, be this as it may, I think it will be generally allowed that the instances which I have produced are both instructive and interesting, as showing that real progress in science, as in everything else, is of slow growth, and that patience is required both in the workers and in those who expect to see or to enjoy the results of their labours.

As we have been treating of the successive advances of instrumental construction from the well made but badly principled quadrants, used during the last century and till the year 1812 in the present, to that triumph of engineering and optical skill the Great Transit Circle of Greenwich, it will be necessary to say a few words with regard to that instrument for the purpose of explaining the grounds of its excellence.

In the first place it may be said that its establishment revolu-

tionized in some degree the whole science of the construction of large instruments for fixed observatories, by transferring all but the purely optical portions from the practical optician to the engineer, and thus providing for the requisite solidity in all the parts which require strength and absolute firmness. All previous instruments made by opticians (Troughton's Mural Circle, for example), were made in a great many pieces connected with exquisite skill, but still liable to derangement and unexpected alteration of adjustment. We may cite as an instance of this (if there are any in the room who remember the early history of the Cape of Good Hope Observatory), the worry and vexation which Jones's Circle caused to the Astronomer, Mr. Fallows. Then again, many things were then done by hand which are now done with infallible accuracy as a matter of common engineer's work by machinery; for example, the planing of steel or brass surfaces.

Well, for the Greenwich Transit Circle, engineer's work (that of Messrs. Ransomes and May) was employed for all the large and solid parts of the instrument, the separate parts being as few as possible, and as much as possible cast in one flow of metal.

Thus, the telescope itself consisted of three pieces; namely, the central cube with its pivots in one flow of metal, and the tube in two portions bolted on to the central cube by means of planed surfaces. The microscopes, formerly the least trustworthy portion of a large instrument, were here the most secure. Their eye-pieces are arranged in a circle, about two feet in diameter, at the back of the pier, and are fastened down by screws, to brass sockets let into the pier, by means of planed surfaces, so that absolute solidity is acquired.

The object glasses of the microscopes on the inside of the pier are rendered absolutely firm by similar arrangements, and it is believed that both in this instrument and in the Carrington Transit Circle used at Oxford, the changes in the course of a year in the relative readings are mainly such as arise from the expansion and contraction of the pier itself.

The zenith points, both as determined by reflexion observations of stars and by the reflected image of the wire, are wonderfully steady for long intervals of time, and whatever faults are at any

time observed in either instrument are due either to inevitable wearing of screws, or some other fault not connected with the original construction.

But probably this is enough to say here about this world-famed instrument, and I may conclude my lecture by a brief summing up of the chief matters which have been under our consideration.

It has been seen that the two elements which were to be determined instrumentally for the purpose of fixing the position of all celestial objects, were the apparent declination and the right ascension, the former corresponding to terrestrial latitude and the latter to terrestrial longtitude. Of these two co-ordinates it has been shown by what slow and successive steps the modern accuracy and convenience have been attained; from the clumsy and ill adapted quadrants of the last century to the almost perfect transit-circle of the present time; from the small four-inch or three-inch object glasses of earlier times, to the large eight-inch refractors used in many places for meridian instruments of the present,—what an immense advance!

Then again with respect to right ascensions, compare the materials at the command of Maskelyne; the small imperfect transit instrument, the almost absolute want of well-determined places of standard stars, and the want of tables for computing the corrections due to precession, nutation, aberration, etc., which are now a matter of daily and common use to all astronomers. Then again, the isolation of astronomy, and indeed of each of the physical sciences, was almost complete. Men, like Maskelyne, devoted to the science, and able, by adequate mathematical and other attainments, to advance it to the next stage of progress, were rare. There was no collision of mind with mind, the great astronomer must work out his problems unassisted and alone, for the only astronomy of that age which attracted public attention was that of Sir W. Herschel, and very few even of *his* admirers were capable of appreciating the labour and profound calculations by which his great discoveries were obtained.

How different, again may we say, is it now! Every branch of physical science is connected with many other branches, and

must ask for their assistance to secure success. Astronomy requires, in its various departments, the labours of the engineer, the chemist, the electrician, the geographer, the geologist, and the mathematician. Glass-workers, since glass became cheap, are required to furnish enormous object glasses, for further discoveries in the heavens; the telegraph-wire determines relative longitudes of observatories; coal mines are descended to determine the earth's mean density; nice mathematical calculations are required for giving shorter focal lengths to our gigantic refractors, and rendering these large instruments at all practicable or manageable. If a new discovery be made, it is claimed frequently by more than one discoverer, and discoveries even of new bodies in the heavens have become matter of almost weekly occurrence.

Let us use these advantages without boasting, and, finally, let us, in gazing at and admiring, some of us one department and some another, of this wonderful collection of scientific instruments got together under this roof, not boast of our advances beyond our scientific predecessors, but modestly endeavour, each in his own degree, to add a mite to what appears to be this infinite sum of knowledge, and remember, finally, that apart from material interests and human needs, it can all have but one ultimate use and end, and that end is the glory of God.

The CHAIRMAN: Ladies and Gentlemen,—It would be quite a work of supererogation for me to ask you to express what you have already done, your thanks to one of our most eminent astronomers for his most laborious and thorough description of the instruments which have been used for the determination of these astronomical constants. But as a matter of form, I will ask you to record your thanks to Mr. Main for his admirable lecture.

HEAT AND WORK.

By Professor Francis Guthrie, LL.B.

July 11th, 1876.

Major Festing, R.E., in the Chair.

The Chairman: Ladies and Gentlemen,—It is my duty this evening to introduce to you a gentleman who is kind enough to give us one of the lectures of this series, Mr. Francis Guthrie, late Principal of the Graaf Reinet College, in Cape Colony. The title of his lecture is Heat and Work.

Professor Guthrie: Among the many examples of apparatus of historic interest which are exhibited in this Loan Collection, there are perhaps few which more deserve your attention than the one you see before you, the apparatus of Dr. Joule, of Manchester, by which he demonstrated or calculated the exact equivalent between work and heat—the mechanical equivalent of heat as it is called. The name is a somewhat awkward one. The word mechanical serves a great many purposes in the English language; we often make use of it when we do not know what word to make use of, and this is an instance of such a use of the word. It would be more correct to call it the thermal equivalent of work. I shall have to explain to you more exactly what we mean by this phrase afterwards; but for the present I may briefly say that by the thermal equivalent of work, we mean the amount of work which will produce a given change of temperature in a given bulk of some standard substance. The determination of the thermal equivalent of work constituted a great step in the theory of applied mechanics. I will not say of so much importance as the establishment of such a theory as that of gravitation in astronomy, but nevertheless we must consider that Dr. Joule's experiments form an epoch in

mechanical science. Of course it has been long known that there was a possibility of converting heat into work, and work into heat. No one knew that better than the celebrated engineer Watt, the whole of whose life was spent you may say in different devices for the most economical conversion of heat into work. But what he did not know, and what Dr. Joule ascertained, is the precise amount of heat which is capable of being converted into work. This, however, is only one branch of the subject which I shall have to treat of to-night. My subject is Heat and Work, and I shall have, therefore, to touch not only on the conversion of heat into work, but likewise on the subject of the modern theories concerning the nature of heat itself.

The ideas of the ancients on the subject of Heat and Work and chemical processes have been already touched on in this room by one much abler than myself, Mr. Lyon Playfair, and, therefore, I shall pass over this part of the subject with brevity. Amongst the ancients we find that the idea of the constitution of matter as being made of small molecules or particles in a rapid state of motion was already entertained. The Roman poet Lucretius, in his celebrated poem on 'The Nature of Things,' accounts for the nature of the physical properties of matter by this theory. We cannot, however, correctly say that Lucretius entertained the modern idea of the atomic theory. His idea of the constitution of matter as composed of atoms was very different from the modern scientific theory. But, nevertheless, his theory is sufficient to show that it is the natural tendency of the human mind, in investigating the properties of matter, to endeavour to explain these properties on some such theory as this. Still less can we say that Lucretius entertained any such idea as is at present held concerning the nature of heat. As I shall have to explain to you, the modern theory of heat is that it consists in the motions of the atoms. Now, we know that amongst the ancients heat was considered in altogether a different way. It was considered to be an element itself, just as much an element as the so-called elements earth, air, and water; and although the chemical ideas of the ancients have been very much modified in modern times, still it is curious to notice that this idea concerning heat as an element has been entertained even you may say up to the present day.

In the early part of the 18th century the celebrated Prussian chemist Stahl started the doctrine of phlogiston. According to this doctrine, heat still remained an element, as it had remained in the opinion of the ancients. Heat played very much the part, according to the doctrine of phlogiston, of a sort of negative oxygen. Where we suppose that a change is produced by the addition of oxygen, Stahl entertained the view that the change is produced by the loss of phlogiston. As soon as the element oxygen was discovered, and its properties were studied, it was seen that this theory of heat was thoroughly untenable. Lavoisier, therefore, started a new theory of heat, but according to this theory heat still remained practically an element; indeed, I believe Lavoisier himself mentions heat under the name of caloric in the list of elements. It is a substance, perhaps not possessing weight, but which is capable of entering into combination with other substances, just in the same way as hydrogen or oxygen can do, and of being separated from them.

Long before Lavoisier's time, however, something like an accurate idea of the nature of the heat had begun to be entertained. In the beginning of the 17th century the celebrated Lord Bacon had made some experiments on the subject of heat, and had come to the conclusion that heat is some form of motion. This contained the germ of the modern theory of heat. Towards the end of the 18th century, Count Rumford, the celebrated American philosopher, entertained some doubts of the existing theory of heat, the phlogiston and caloric theory, for the phlogiston theory had not then quite given way to the theory of caloric. His attention was first called to this subject in boring out a large cannon. He noticed that the temperature of the metal rose, very much; that the work done by the boring-tool seemed in this way to be converted into heat. The explanation which was then given was this, that a separation of the parts of the metal from the mass set free a certain amount of caloric which exhibited itself in the form of heat.

Count Rumford was not satisfied with this explanation, and he tried the following experiment. He tried whether the metal when separated into small fragments possessed the same specific heat as

it did in the mass. He found that it did; that there was the same specific heat in the small fragments as there was in the entire mass, and it occurred to him therefore, that it was utterly impossible that heat could be set free by the disintegration of the metallic mass. Sir Humphry Davy, almost at the same time, or a little subsequently, tried the following experiment. In the vacuum under an air-pump he rubbed together two pieces of ice, and found they were speedily melted and became converted into water. Since the space in which the ice was contained was a vacuum, he was pretty certain that no heat was conveyed to the ice by conduction. A small amount might be conveyed by radiation, but not nearly sufficient to produce the effect. The specific heat of water is much greater than that of ice, and, therefore, there must have been the accumulation of a considerable amount of heat, much greater than could be accounted for by radiation. This, therefore, appeared like the creation of heat. Now, if heat was a substance as was supposed, this was contrary to the analogy of all other species of matter, and, therefore, threw considerable doubt upon the caloric theory of heat.

The celebrated Dr. Thomas Young likewise investigated this subject, and with his usual acuteness he arrived, I believe, at the same conclusion which, I suppose, now is generally entertained, in opposition to the notion of the time, that heat is a form of motion, and that there is no such thing as a separate substance called caloric. The subject, however, could not be considered to be finally settled until experiments were made by which the actual calculable equivalents of heat and work were ascertained. This was done by Joule, of Manchester, and about the same time Mayer, of Heilbronn, was investigating the subject, so that both are entitled to equal honour, I believe, on this point.

The apparatus you see before you was the one actually used by Dr. Joule. This is a copper vessel in which liquid was placed, either water or any other which required to be investigated. In this copper case you will observe that a spindle with a good many paddles is inserted in such a way that, as it rotates, the liquid is necessarily very much disturbed by friction. It is fastened to an axis or spindle which can be made to rotate by drawing out this

thread. The thread was passed over the pulley, a weight of known amount was fixed to the lower end of the string, and the spindle was placed in this vessel. The paddles were then caused to rotate by the descent of the weight, and the temperature of the water or other liquid was ascertained before and after the rotation. Several other precautions were taken which I need not here mention, but the main point of the calculation was this, that when a given weight descended through a given space, the temperature of a given weight of water in this copper vessel was raised a certain number of degrees. The rise of temperature was found to be proportioned to the weight and to the space through which it descended, that is to say, to the amount of work done. Chemists had already calculated the relative specific heats, as they are called, of different liquids; and when the relative change of temperature of one liquid, such as water, with the amount of work done was ascertained, similar changes of temperature which would be produced on other liquids would be easily calculated, and were confirmed by experiment. The experiment is altogether too delicate for me to show in a lecture-room; it would take too long a time and far too many precautions, but by somewhat modifying the experiment you can see in a rough way the production of heat by work and thus verify the main principles of Joule's experiment. Here is a common whirling table with an upright tube into which a small quantity of ether is poured. When the wheel is turned round, the tube of course rotates on its own axis, and it is clasped by a wooden clip. You see a certain amount of force is required in order to turn the wheel, and that travels over a certain space as the wheel is turned; that is to say, it does a certain amount of work. Part of the work is consumed entirely in the friction of the machinery—friction which we cannot avoid, though we would do so if we possibly could. The remaining portion of work is consumed in the friction generated between this wooden clip and the metal tube, and that friction is work which is being converted into heat. After a time the heat will become sufficiently great to boil the ether, and the tension of the vapour of ether will no doubt project the cork from the tube. If we know the specific heat and weight of ether here employed, a simple calculation will show

the amount of force generated; thus we can ascertain the amount of work required to boil that particular weight of ether. As a matter of fact this experiment is much too coarse, because a large amount of heat escapes by conduction from the different objects in contact.

I.

FORCE.

Rate of change of momentum.

Mass times acceleration.

$$f = m\,a$$

Unit, a Dyne; a gram accelerated a centimeter a second.

II.

WORK.

Force displacement.

Force times its displacement in its direction.

$$w = fs$$

Unit, an Erg.

Work is either positive or negative.

III.

POTENTIAL.

Possible work of a force under given conditions.

IV.

KINETIC ENERGY.

Mass motion produced by work.

Mass times half the square of the velocity.

$$k = m\frac{v^2}{2}$$

V.

MASS ENERGY.

The kinetic energy of a body, due to the common motion of the molecules.

VI.

MOLECULAR ENERGY OR HEAT.

The kinetic energy of the molecules due to their relative motions.

Before proceeding to state to you more definitely what is the modern theory of heat and matter, I must call your attention to

these few definitions. The first is a definition of what we mean by force. Force is the rate of change of momentum or mass times acceleration. This is not exactly the ordinary definition of force. Force is rather defined as the cause of change of momentum, the cause of the acceleration of a given mass; and it is said we measure force by the amount of acceleration it produces on a given mass, and by the mass on which it produces a given acceleration. As a matter of fact, however, we have nothing to do in science but with the effects caused by acceleration, and the mass on which it produces the acceleration; we have nothing to do with this unknown cause, and the sooner the idea of this unknown cause is left out of the determination of force the better for science. It interferes very seriously with the proper understanding of the subject. The idea of a cause for acceleration becomes associated in the mind with muscular exertion and various other forms of force which only tend to bring confusion into the subject. Although the definition is not exactly the same, therefore, as that ordinarily accepted, it is practically identical. Force is mass times acceleration. Work, as I have already had occasion to mention, is force times the displacement in its own direction. If the point of application of force is moved in such a way that either the whole or part of its motion is in the direction of the force itself, then that force is said to do work. Here again there is a little confusion in the way in which the term work is used. Sometimes it is used with reference to force overcome, and sometimes in reference to the displacement of the force in its direction; sometimes the amount of displacement of force in a direction opposite to its own, and sometimes its displacement in its own direction. Of the two I prefer to accept the latter definition. Either would come to the same thing. It follows from this that the work is always equal to the product of the force and the space through which it is exerted.

The unit of space which is now used in science is the meter or centimeter generally, and the unit of mass the gram; the unit of force, that amount of acceleration which would produce a velocity of one centimeter per second in a gram of matter in one second. The unit of work has had the name "*erg*" given to it. Work, as I have had occasion to mention, is positive or negative. When the

force is moving so that its point of application moves in a direction opposite to its own, then we say the work of force is negative; if in the same direction as itself, we say it is positive. The term "potential" has been comparatively recently introduced, and it has been found so useful that no doubt it will come into general acceptation. By potential, we mean the possible work of a force under given conditions. Thus, for example, taking the weight which is attached to an ordinary clock, that weight is capable of descending through a certain space, viz., the length of the chain which is wound round the barrel. The potential of that force then is the weight, multiplied by the length of the chain; it is the work which the weight is capable of doing in moving the clock. You will easily see that potential is the complementary to work, and when work is done potential is diminished. If the force is exerted in its own direction it has less distance to be exerted. If that force alone acts upon a body, from the very nature of force (force implying acceleration), the body acquires velocity; and it can be easily shown mathematically that the amount of velocity obtained by a body, when a certain amount of work is done, is such that the force times the space through which it acts is equal to the mass times half square of the velocity, $fs = m\frac{v^2}{2}$ A name is required for that amount of motion, and the name kinetic energy has been given to it, $k = m\frac{v^2}{2}$

Formerly the same quantity was known as *vis viva*, but that is a somewhat awkward phrase, and the term kinetic energy has been substituted for it. If the Greek language would admit of a combination I should suggest shortening it into kinergy, and then we should likewise have a name for the unit of kinetic energy, namely, kinerg, in the same way as the unit of work is called "erg."

Kinetic energy is twofold, and may be divided into mass-energy and molecular-energy.

Those two phrases are sufficiently explained, I think, on the diagram; mass energy means the kinetic energy of a body due to those motions which are common to the molecules of the body; by the molecular energy we mean the kinetic energy of the

molecules due to their relative motions. You will see, however, that there is no broad line of distinction between these two, but that mass energy gradually modifies away into molecular energy. First of all, we take the case of pure translation, where every point in a body moves in the same direction at the same time, that would be pure mass energy; if we take the case of rotation where every point in the body has its own direction, but where the points near to each other have a direction almost coincident, we still have what is called mass energy, but somewhat approaching to molecular energy. If we take the case of vibration, as in the case of waves in the air, we have still what is called mass energy, but still more closely approaching to molecular energy. If the motions of the number of waves be increased without limit and their directions varied without limit, finally every single molecule would have its own particular direction of motion, and the mass energy would ultimately become molecular energy.

With these definitions I shall now proceed to state what is the modern theory of heat, work, and matter. Matter is supposed to consist of small molecules separated from each other, each having its own peculiar existence, as it were, similar to each other in weight and motion—not necessarily, because each might possess its own proper motion. The different states of aggregation of matter are explained as follows. In the case of a gas the molecules are supposed to be relatively at a considerable distance from each other and moving freely amongst each other, their motions being only occasionally influenced by each other when they come in contact, or perhaps in the slightest degree by molecular forces. But in a gas the particles are supposed to be for the greatest part of their time moving freely; the periods of time when they are in actual contact with each other are supposed to be very short, and molecular force is supposed to produce comparatively little effect. In the case of a liquid or solid, the molecules are supposed to be much nearer together, their motions to be more confined, but they are still supposed to possess motions either of vibration or rotation, and probably to move even in small orbits, and return rapidly to the same place.

Heat, as I have already stated to you, is the motion of the par-

ticles—the motion of the particles measured by this formula (IV). Multiplying the mass of each molecule by the square of the velocity divided by two, and adding all these products together, we get what is called the heat of the entire mass.

You will see that the particles of a body are capable of three distinct kinds of motion, namely, motions of translation, motions of rotation, and motions of vibration. Particles of matter are supposed to be rotating on their axes, and likewise to be in a state of vibration; we suppose them to be continually encountering each other; and you know the effect of elastic bodies striking each other is that they are put into a state of vibration. You will easily see all three of these motions in ordinary billiard balls as they roll about the billiard table. They travel along, they rotate on their axes, and when they strike together the sound of the impact shows vibration in the ball itself. It is true that this vibration soon ceases owing to friction; but in the case of the molecules you must suppose that these vibrations are as permanent as the motion of rotation and translation.

Now let us see what would be the physical effects of these molecular motions; and for simplicity we shall take the case of a gas where the motions are less influenced by molecular forces. A gas may be compared to a billiard board on which there are a number of billiard balls moving with very great velocity, and continually encountering each other; and necessarily therefore encountering the sides of the table. The balls are supposed to be perfectly elastic, and the cushions of the table also, so that their motions when once set up, continue without diminution. One effect, you will easily see, will be this, that a continued pressure will be exerted on the cushions of the billiard table, and if the cushions, instead of being affixed to the table, were so attached to it that they were capable of motion, the continual impact of the balls would gradually drive the cushions backwards and expand the table. That is precisely what we find to be the case with a gas. If we enclose a gas in a vessel, and allow one of its sides to be movable, and if there is no pressure on the outside of that movable side, then it will be driven along by the pressure of the gas inside, just in the same way as the impact

of the billiard balls would drive the cushion along if it were free to move. But you will notice that the effect on the cushion of the billiard table is due to the motion of translation of the billiard balls, and not to their motions of rotation or vibration. It is only one portion, therefore, of the kinetic energy of the molecules of the gas which produces pressure on the sides of the vessel in which it is contained; namely, the motions of translation. It can be easily shown by a very simple calculation that the pressure which would be produced on the sides of the vessel, due to the impact of the molecules, would be such that calling the pressure on the square unit p, $p=\frac{2}{3}$ of k, where k is the kinetic energy of the molecules, due to translation only: the pressure on a unit of surface is $\frac{2}{3}$ of the amount of the kinetic energy due to translation, in a cubic unit of the gas. This enables us to come to this remarkable conclusion, that we can ascertain the actual velocity of the motions of gas.

Take the atmosphere for example. We know what p is and we know that $k = m\frac{v^2}{2}$ and from that we easily get $v=\sqrt{\frac{3p}{m}}$.

We know what the pressure on a square unit of surface is, and we know what mass of a cubic unit of the atmosphere is, and we can therefore find what v is, the velocity of the motion of the particles of air. It comes to somewhere between 1400 and 1500 feet a second—about a third more than the velocity of sound. The minute particles of air therefore, although the air seems perfectly still, are moving according to this theory (and of its truth there can be very little reasonable doubt now) with a velocity of about 1500 feet a second. The spaces through which they move before their direction varies are extremely small, and therefore the number of particles in a given mass of air is immensely great, and you can easily suppose therefore that an immense number of impacts are at any given time occurring on any particular portion of the surface. But although the particles themselves are so minute, still the number of these impacts and the velocity of the motion of the particles is so great that the pressure on the side of the vessel is, what we actually know it to be, very considerable. In the case of an atmosphere of ordinary density it is about 15 pounds on the square inch.

Another result of this theory would be as follows :—supposing that this movable side of the vessel were forced inwards, what effect would that have on the motion of the particles inside? You know that if a cricket ball strikes the bat, it rebounds with a velocity equal to its velocity of impact; that is to say, it would do so supposing it were perfectly elastic. If the bat is at rest the ball rebounds with its original velocity, but if the bat is itself in motion in the opposite direction, the ball will rebound with a velocity increased by double the velocity of the bat. Precisely the same thing will occur with respect to this wall of the vessel. If it is at rest the particles which impinge upon it will rebound without loss of velocity; but if this wall is in motion towards the mass of the gas, the velocity of each particle will be increased by double the velocity with which the wall is moved. But since velocity is heat, the heat of these particles will be increased. While the side of the vessel is moving, practically an infinite number of particles will impinge upon the side of the vessel and have their velocity increased, and we should expect, therefore, to find that when a portion of gas is compressed its heat will be increased; and if, on the other hand, the side of the vessel is moving away from the mass of the gas, then the velocity of each particle on impinging will be diminished by twice the velocity of that side which moves, and we should expect, therefore, to find that during expansion, the temperature of a confined portion of air would decrease, and the amount of increase and of decrease might always be expected to be as follows. Calculate what the pressure is on this side. Let this side be a unit of surface, and call the distance through which it is compressed x. The work done by the amount of pressure would be $p\,x$. It would at first sight appear therefore that the temperature of the gas contained in the vessel would be increased by the quantity $p\,x$. Now, that it is increased we know perfectly well, and when air is compressed its temperature rises; and that its temperature falls when it expands is likewise true. But that the increase of temperature is exactly equal to the work done during compression is not exactly true, for the reason you will see presently. It is perfectly in accordance with theory that it should not be so. No doubt most of you have seen these experiments of the heat pro-

duced by the compression of air. This is a glass cylinder into which a piston fits air-tight, and on the end of the piston there is a small portion of German tinder. If the air be suddenly compressed by driving the cylinder down on the piston, the heat accumulated in the compressed portion of the air will be sufficiently great to light the tinder. Again a damp atmosphere is secured in the receiver of this air-pump by inserting a damp sponge into it. The air will be expanded by rapidly working the air-pump, and the effect of suddenly cooling that moist damp air would probably be to condense a portion of the vapour of water into air in the form of a cloud. We know that clouds are produced in that way. The air becomes partly filled with dense vapour on a rapid fall of temperature. Now I was telling you that the change of temperature does not come out precisely as we should at first sight have expected it. If the whole of the work done on the air under compression was converted into motion of translation, the change of temperature would be greater than we actually find it to be, but you can easily see that this is impossible. Taking the simile that I took before of a number of billiard balls moving about and striking each other; then according to their velocity a portion of their motion will be converted into motion of vibration and a portion into motion of translation. The total kinetic energy will be divisible into these three parts—the motion of translation, motion of rotation, and motion of vibration, and these three parts you can easily see will likewise be in a pretty constant proportion to each other. If we increase the motion of translation, we shall also increase the motion of rotation and of vibration. Now pressure is due only to motion of translation; if therefore we increase the motion of translation, only a portion of that motion of translation will be converted into motion of rotation and vibration, and therefore we shall find that the increase of temperature due to pressure will not be so great as it would be if the whole of the force had been expended in increasing the motion of translation. It is found that about three-fifths of the entire amount of work done, is spent in increasing the motion of translation, and therefore increasing the pressure on the side of the vessel, and the remaining two-fifths is spent in increasing the motions of rotation and vibration. We may say, therefore, pretty confidently that the

whole molecular kinetic energy of the mass may be divided roughly into these two portions, three-fifths due to motion of translation, and two-fifths due to the other motions of vibration and rotation.

Now with these definitions of motion, of heat, and of material constitution, we may come to the essential law of the active conversion of heat and force, which is this, that throughout all nature the sum total of potential and kinetic energy is a constant quantity. Wherever the potential is decreased, there is an equivalent increase of kinetic energy; and wherever kinetic energy is decreased, there is an equivalent increase of potential. I would take an instance to illustrate this conversion of potential and kinetic energy. Imagine that a musket is discharged perpendicularly upwards. The first part of the operation is the conversion of the particles of gunpowder into a gas. The particles of gunpowder are all in a state of unstable equilibrium, and when heat is applied to them they become converted into a gas. This gas is in a state of very great density compared with the air, and its temperature is very high. The consequence is that the pressure or force on the lower part of the bullet is very much greater than the pressure of the atmosphere on the other side, and the bullet is therefore driven with a constantly increasing velocity along the barrel of the musket and escapes from the muzzle with a velocity, in an ordinary musket, of more than a thousand feet per second. While the bullet is being driven up the musket, the kinetic energy of the particles of the gas is being communicated to the bullet itself. The molecular kinetic energy is being converted into mass kinetic energy. When a bullet issues from the mouth of the musket it possesses a considerable amount of kinetic energy, $m\left(\frac{v^2}{2}\right)$. As it ascends, it increases the potential of gravity and at the same time it is overcoming the resistance of the air; in so doing there is what is called friction, and this friction, as I have already shown you, produces heat, which is molecular motion. You have therefore two things going on: the kinetic energy of the bullet when it leaves the muzzle of the gun is being converted into potential of gravity and into molecular kinetic energy, the molecular kinetic energy of the musket bullet and the air. As the bullet ascends, its velocity

decreases until at last it comes to rest; the whole of the kinetic mass energy has then been taken out of it, and has been entirely converted into potential of gravity; that is, the bullet raised to a height from which it is capable of falling, and molecular kinetic energy. It then begins to descend. The potential of gravity becomes gradually reconverted into kinetic energy; part of it again is abstracted in the form of resistance and friction, and takes the form of molecular kinetic energy, and ultimately the bullet falls to the ground. We will suppose that it falls upon an iron plate so that its motion is suddenly resisted. If it falls with sufficient velocity, a leaden bullet will become immediately melted; and this is an instance of the immediate conversion of mass kinetic energy into molecular kinetic energy; the common motion of the molecules of the bullet which they had in descending, becomes suddenly converted into a relative motion amongst each other, which is heat, and, therefore, when the heat is sufficiently great the lead is melted.

Another good illustration of the law is a consideration of the nature of engines, which are constructed for the purpose of converting heat into work. The ordinary steam-engine is an engine of this description. A certain amount of heat is available by the combustion of fuel. The amount of heat produced by a known amount of carbon and a given amount of oxygen is easily calculable; this heat is devoted to raising the temperature of water above the boiling point, and converting a portion of it into steam at high pressure of considerable density and considerable temperature. If that steam be let in below the piston of a steam-engine, the pressure on the upper part being less than that below, the piston is driven upwards with a certain amount of force, and that force is turned to any purposes for which it is required, either to propel a steamer or a locomotive or to work machinery, so that you see a steam-engine is used for converting heat into work.

The principle of the steam-engine, however, is somewhat complicated by the fact that it involves the change of the state of aggregation of a substance, the change of liquid into gas and again of gas into liquid. The principle of the *heat* engine is more easily understood from a consideration of the nature of atmospheric engines.

Imagine we have two cylinders, each with a piston fastened to a common piston rod at such a distance apart that when one presses one piston up to the end of the other cylinder, there is no compression in the first one. Imagine that the amounts of gas in both cylinders are the same, but that in one cylinder it is partly compressed and in the other not. The density in the first we call d_1, and in the other d_2. When the piston is driven to the opposite end of the first cylinder the air in it will be partly compressed and the air in the second one will be fully expanded, but when it is driven back again the second becomes partly compressed and the first one fully expanded. If the temperature be the same in both, and remain the same, the work of compression would be exactly equal to the work of expansion, but if the temperatures are different, it follows, by the well-known laws of gases, that the amount of work will be in each case proportionate to the temperature. Call the temperature of the first t_1, and of the second t_2; and call the work required to compress the one w_1 and the other w_2. Then $w_1 = a\,t_1$. By the temperatures, I mean the absolute temperatures. Not the temperature above the freezing point of water, but the temperature above a point 273° below the freezing point of water. The reason for choosing that particular temperature, and calling the temperature above it the absolute temperature I shall not be able to explain to you now, but you must understand for the present that we chose that for the sake of simplifying our formulæ. The work then will be proportionate to the absolute temperature, $w_2 = a\,t_2$; when I press the piston to this end I am doing work in compressing this gas, but the expansion of the gas in the other cylinder assists me. The actual work I employ, therefore, will be $w_1 - w_2$. The actual heat which I generate in the end of the cylinder will be proportionate to the work I employ upon it, namely, w_2, and the actual heat which is absorbed will be the work it does, namely, w_2 or a times t_2; consequently you will see that this equation comes out, that the work that I have to employ is the difference of these two. The heat which I impart to the left-hand cylinder is abstracted from the right-hand, and calling the heat H $\frac{H_1 - H_2}{H_1} = \frac{w_1 - w_2}{w_1} = \frac{t_1 - t_2}{t_1}$ when the right-hand gas is com-

pressed and the left is expanded, I have practically come back to the same condition from which I started, because these two masses being the same I can, without doing any work, interchange their temperatures and give back to this one its old temperature, and to that one its old temperature. Therefore it follows that the amount of work that is actually done is to the amount of work which is imparted to this cylinder as the difference of t_1, and t_2 is to t_1, and the amount of work imparted to this cylinder is equal to the amount of heat it received; call one H_1 and the other H_2 and we have the following results; that in order to produce a given amount of work w, we must expend an amount of heat such that $w = H \frac{t_1 - t_2}{t_1}$. The amount of work we can get is not equal to the amount of heat we are obliged to employ, but is equal to this fraction of it, beginning with the absolute temperature of the two gases, and being smaller in proportion as those two temperatures are nearer to each other. This is the true theory of all atmospheric engines; the amount of work you can get from an atmospheric engine is not in proportion to the amount of heat which is available, but depends likewise upon the temperature of the medium in which you are working, into which, in fact, you have to discharge the heat. You cannot convert heat into work directly—not the whole of the heat; you have to allow a certain amount of heat to pass into another medium, and the fraction which you can actually turn into work depends upon the difference of temperature of the two media, and is smaller in proportion as the two media are of the same proportion. It is an advantage always, therefore, to work engines at high temperatures and discharge the heat afterwards at low temperatures, because the amount of available force is great in proportion to the difference between the temperature of the heat you are using and the medium into which you have ultimately to discharge your heated steam—the temperature, in fact, of your boiler and your condenser. Subtract the temperature of the condenser from the temperature of the boiler—the absolute temperature—and divide by the temperature of the boiler, and you get the proportion of your heat which you can practically turn into work, and that is the theoretical limit of the efficiency of the steam-engine.

A still more interesting example of the application of heat to work is seen in the case of what is called the thermo-electric-pile. That consists of two unlike metals which are joined together in such a way that heat can be applied to their point of junction, and then an electric or galvanic current would pass from the two free ends through a conductor. I cannot enter into the nature of the galvanic current, but it is some form of force and is capable of doing work, and is therefore kinetic energy—it is either a potential or kinetic energy. It is capable of doing work because when heat is applied the current passes, and work can be done, and we are perfectly certain therefore that a certain amount of heat applied to the point of junction between two metals is converted into work which is actually done. Here is a thermo-electric-pile which, instead of consisting of two elements, contains a number joined together like the elements of a galvanic battery in such a way that the current is increased by their joint action. One of the effects of a galvanic current is that when it passes in a particular direction with reference to a magnetic needle, it has the power to change the direction of that needle in opposition to the magnetism of the earth. Now when a needle's position is changed in opposition to the magnetism of the earth, potential is created; because the magnetism of the earth has the power of bringing a needle back again into its old position. This, therefore, is a simple means of showing that heat can be directly converted into work. If this end of the thermo-pile be warmed, you will see that the needle will begin to move—the mere warmth of my finger is sufficient to set the needle in motion against the force of the magnet, and that is to produce potential or to do work. The effect of heat in producing work can likewise be illustrated at the same time as the effect of compression and friction upon gas. When that needle is again brought to rest, by working these bellows the air is compressed and the temperature rises; but when it issues from the nozzle, there it expands and cools. If no heat were lost, the temperature at which it issues from the nozzle should be the same as the temperature of the outside air, but as a certain amount of heat is gained by friction in passing through the nozzle, you will find that the actual temperature of the issuing air is some-

what greater than that of the atmosphere, and a motion therefore will be produced in the needle, though the effect is but very slight. Here again is some air, in a copper vessel, which has been compressed. After it has had time to cool, then its temperature on expanding will fall below that of the atmosphere. When allowed to issue from this orifice, it will be slightly warmed again by friction, but still its temperature will remain lower than that of the air, and in this case the needle moves in the opposite direction.

Another interesting instance of the conversion of heat indirectly into work can be shown in the case of what are called electro-magnetic engines, of which I have a sample here. This is not exactly the conversion of heat into work, but it is the conversion of something that we know, by other reasons, to be the equivalent of heat, into work. In a galvanic battery a certain portion of metal is oxidised, a portion of zinc enters into combination with oxygen. If this process goes on directly under ordinary circumstances, it produces an equivalent amount of heat precisely in the same way as the combination of carbon and oxygen in ordinary combustion. If, however, the oxygen unites with the zinc in particular conditions, as in this galvanic battery, then a portion of that which otherwise would be heat is converted into a galvanic current. The amount of the current is a precise equivalent of the amount of heat which would otherwise be produced by the combination of the oxygen with the zinc, and this current is capable in turn of being converted into work by an arrangement such as this. If the galvanic current is passed round a piece of soft iron, the iron becomes converted into a magnet, and the polarity of the magnet depends on the direction in which the current passes round the iron; when the current passes in one direction, one end would be north and the other south, and when it passes in the opposite direction the first end would be south and the other north. I cannot enter into the details of the construction of this machine but briefly you will understand it to be this, that the currents are arranged so that when we produce motion in the machine certain pieces of iron in these coils become converted into magnets, and the magnetism is destroyed as this wheel

moves round. In one position these magnets attract the keeper, and in another position they cease to attract it. When the piece of iron or the keeper is opposite the coils, then the current gets broken in such a way that they cease to attract it; and when it is approaching, then the currents are so arranged that they attract it. In that way the current becomes converted into magnetic power capable as you see of pumping up water. The actual available power from such a contrivance is extremely small, and for the present there is very little hope of seeing this becoming of any use in mechanics unless some discoveries are made which will render the production of currents cheaper, and the amount of force available from them more considerable.

The great objection to them is the relatively high price of the substance which is consumed in order to produce the force. Zinc, in fact, is burned in order to produce this force instead of coal, and zinc is many times the cost of coal, while the amount of heat, or the equivalent of heat which it evolves in this way is very inconsiderable as compared with the heat evolved from the consumption of carbon.

The CHAIRMAN: I am sure you will all join me in returning your thanks to Professor Guthrie for the extremely interesting lecture he has given us, and for the very lucid way in which he has explained his subject.

MODES OF ELICITING AND REINFORCING SOUND.

By Dr. Stone.

July 15th.

Mr. W. Chappell in the Chair.

The Chairman: Ladies and Gentlemen,—You are well aware that these Lectures are in connection with the Exhibition, and that they are given by eminent men of science gratuitously. We shall have the great good fortune to-night to hear Dr. Stone, F.R.C.P., as eminent in science as he is in his own profession. The subject of the lecture will be strings, monochords, sirens, and the electrical transmission of sound. I can only say that all Dr. Stone's Lectures I have had the pleasure of listening to, I have found not only instructive but most entertaining.

Dr. Stone: Mr. Chairman, Ladies and Gentlemen,—In the few comments upon musical vibration which I propose giving this evening, I shall follow the excellent, I may say the unimprovable, classification of page 18 of the Hand-book. It is almost impertinent for a person like myself to make this remark; for the introductory observations in that book come from the hand of no less a man than Professor Clerk Maxwell, a man who probably, with Helmholtz, stands highest in the rank of physicists in Europe. He says, speaking of machines generally, particularly with regard to acoustical machines, that we have to consider vibrations and waves and the source of those vibrations. He puts first what, by a pardonable metathesis, you will allow me to put last, the vibrations in air. He considers in order strings, membranes, plates, rods, and what he terms distributors, and among distributors air. It is of course impossible

in the short time at my disposal (although I propose, and I tell you so frankly at the beginning, to tax your patience very considerably in case the experiments should require a little time), to give an outline of all the modes of producing sound. It therefore occurred to me that I should do well to bring before you to-night certain less familiar modes; certain more scientific methods —methods which do not come into the range of music; what one may term the experimental modes of eliciting and measuring sound.

I will leave the musical portion out—a portion which you very naturally would appreciate more, and which I should have pleasure in dealing with—till another opportunity; but this evening I hope you will allow me first to make a few remarks on monochords, rods, tuning-forks, and sirens. There is a second part, which I hope the inexorable clock will allow me to give you, upon resonators or modes of transmitting sound, telephones for instance; perhaps I might have specified more distinctly in the title of the lecture what exactly I intended to state. I find in Professor Maxwell's excellent Hand-book that distributors are placed in the same classification, and rightly so, with resonators; therefore the subject of my lecture will rather be, technically given, Producers and Distributors.

Before I proceed to speak of any of these modes of producing or distributing sound, I feel bound to make a few remarks on vibration itself. I have to deal with the most trivial, the lowest, the meanest, the simplest form of vibration with which physicists are occupied. In return, I believe it is a form of vibration which has more power of causing emotion, more power of stirring the heart, more power of changing the character, more power of making a man a good man, a thoroughly honest and friendly man, an appreciative man, a resonant man, if you like to say so, than any other form of vibration. Now this coarse vibration I have to speak of to-day—is merely vibration in the ordinary atmosphere in which we all live and breathe and have our being; it is not a vibration in ether, in an imaginary fluid—I will hardly say imaginary, but perhaps I may say a hypothetical fluid—it is in a tangible substance which we all want 15 or 16 times a minute and without

which we cannot live. We touch very closely here upon light, but we need not go into those more recondite problems — the vibrations coincide indeed, but they also coincide with the pendular vibrations we all know very well as mathematical theorists. What I want to show you first, if I possibly can, is the nature of vibration compounded in different ways, and which rises from the form which we have to discuss to-night up to higher and grander forms which those who work with light—and which perhaps those some day will include who have to deal with that still more subtle vibration (for vibration I believe it to be), Electricity. We have in this Exhibition many means of demonstrating harmonic motions. Several of these will come under the notice of a more competent person than myself, Prof. Barrett, of Dublin, who proposes to show you their composition. All I shall hope to do is to demonstrate the composition of harmonic motion in two instances; in the first place in the case of a string, and in the second place — if I can accomplish it, but I must admit here from the beginning that the accomplishment of this my wish is difficult — in the shape of light, as thrown from vibrating reeds. As regards simple vibrations I have very little to fear. There is here, from a Swedish source, a mode of showing it which is almost if not entirely perfect. You must remember that waves of sound are alternate periods of rarefaction and condensation, and, unlike light, they are not transverse to the medium in which they are transmitted, but they are directly in the course of the ray,— as I speak to you every vibration is going in a series of alternate condensations and rarefactions from my inefficient mouth to your very efficient ears. This we can more or less reproduce by mechanical means, but I have never seen it nearly so well as in this machine.

Here are a number of levers actuated by cams, and a means of turning them at different rates. If time allowed me I would dilate upon its construction, because it is very interesting and contains the whole essence of theory, but you must permit me on this occasion to show you merely what happens. To my mind it is much more than a mere demonstration. I

seem to see, as my eyes look over it, the alternate periods of condensation and rarefaction, and it is these vibrations which I hope in one form, and that only the roughest, to bring before you; but this Swedish machine, which I believe is due to Baron Von Riel, gives you a most admirable idea of sound vibration. At one end of the line is the speaker, and at the other the hearer, and between speaker and hearer go these various lines representing the layers of condensation and rarefaction.

After this I can go one stage farther in speaking of pure vibrations, by showing you the vibration of a string under one or two impulses. Here is a lax string which is put in front of a black screen, so that you may the more easily see it, and I have the power of setting one end of it, indeed both ends, into vibration by means of electricity. The string is vibrating under the influence of a spring driven by galvanism and the galvanic current forming the vibration is of a less period than that of the string itself. Therefore if you look closely, you will find the string is divided by a node in the middle. There is a very large vibration at this end, then comes the node, and at the other end similar vibration; in fact, that string is speaking its octave; it is forming a harmonic in the middle. But I can do more; I can produce in the string a second vibration, of which I have the power of altering the plane. The string is still vibrating in two segments, but I can turn this end round in a rectangular direction, and now instead of producing as it was before linear, it is producing elliptical vibrations. If you look at it now you will see it is forming elliptical figures, the ellipse being equivalent in the major and minor axes to the powers I am using at the two ends. I shall have to refer to this experiment later on.

I have next to speak of strings. Strings were the earliest source of sound used. In fact, they were used infinitely more early in the world's history for acoustic experiments than any other mode of producing sound. Old tradition assigns these determinations from the length of the string to Pythagoras, but our excellent chairman Mr. Chappell, whose erudition in matters

of ancient music is beyond that of any man living, has shown that Pythagoras did not so much originate the idea of the vibration of strings as he imported it from Egypt, or perhaps even from Babylon. No doubt in ancient times, for the antiquity of Egypt and Babylon is very far antecedent to anything we in these modern days can trace, many such discoveries were made, lost, and forgotten, and were then re-discovered in later times. I should be sorry to speak lightly of so honoured a name as that of Pythagoras, but I entirely believe what Mr. Chappell has proved; that Pythagoras having, as is admitted on all hands, travelled in the East, had there gathered many forms of science, and amongst others the divisions of the octave. Certainly in Euclid's time—but in Euclid's time we are speaking of a rather late period, because Euclid, great as he was as a mathematician, as a classic, is somewhat late—it is recorded that the *sectio canonis*, which meant the monochord, was well known and distributed over Greece. But no doubt Greece borrowed from her Oriental predecessors. Now, taking the monochord as we find it in Euclid's time, we find a very great advance. Every one probably knows that the pitch of a string is inversely to its length; it is inversely as the square root of its tension, and to this I shall have to advert again. It is inversely as the square root of its mass; and the forces which stretch the string are proportional to the sectional area of such a string. This goes rather beyond our present subject, although I hope to take it up when I speak before a class of science teachers. The relation of length, however, we may very well consider here. Probably most of you know that if you halve a string in the ratio one to two, you get the octave; if you take the ratio of 2 to 3, you get the 5th; if you take the ratio of 3 to 4, you get the 4th. The ratio of 4 to 5 gives you the major third, and the ratio of 5 to 6, the minor third. Now here we have all the important intervals of the octave in a very easily recollected form, 1 to 2, 2 to 3, 3 to 4, 4 to 5, and 5 to 6. It is true that these are not all the intervals of the octave, but all the other intervals may be obtained by inversion. The 4th

is an inverted 5th; that is very simple, you merely exchange the figures; the minor 6th is an inverted major third, and you take 5 to 8; the major 6th is the inverted minor 3rd, and you take the ratio of 3 to 5. These are all the consonant intervals in an octave.

As to how far these have been experimentally and instrumentally demonstrated is the next point. Euclid had the monochord; it is not a very difficult thing to make. He had created the method by which he was able to work out all these determinations; but in modern times until the time of Galileo, as you will see given in Mr. Chappell's book, the question seems to have slumbered. Galileo was certainly never brought before the inquisition for his division of the octave, although he was for other things; but his division of the octave has remained as true, as great, as eternal, as much a matter of human learning granted by the powers on high, as though he had never been assailed by any inquisition on the face of the globe. However, we have a long gap from Euclid to Galileo, and then comes another long gap until this monochord was practically utilised. I think I may say that the first instrument which practically utilised the monochord is one shown in the Exhibition below, but unfortunately it being a private loan, I am not able to exhibit it here to-night. I mean the monochord of Broderick and Longman, which dates from more than 100 years ago, and is recommended to private players, as a means of tuning their harpsichord. That shows very well the antiquity of it. A most interesting mode of getting the string divided according to the regular division of the octave it is in practice, and you could by a moderate amount of ear have tuned your harpsichord very much better than it would have been done by any perambulating tuners of those days; but we can now get more accuracy than that.

Then I have to name the father of enharmonic music, Perronet Thomson; who, from first to last in the fabrication of his magnificent instrument—for in its day it was magnificent and it remains sole and single to this day—preferred to use the monochord; not in the ordinary form in which the tension is obtained by means of wrest pins

and pressures which you cannot measure, depending on the muscular force of the arm, but on the use of weights. I believe the weighted monochord is still the best mode of obtaining on demand a given number of vibrations in a certain time, that is to say, in a second. This I could not without much more time than is at my disposal demonstrate to you, but I can illustrate it. The weight which is to stretch the cord rises by squares, not in natural numbers; so you have always an advantage in stretching a string by a weight because its errors will be measured by their square roots, so they will be small quantities. I have here a thin string stretched by a small weight, so small that it hardly gives a musical tone. I add one graduated weight to it and it goes up a little, but it is still a moderate tone. Remember if I were screwing away here at this pin, I should be raising it most rapidly and without knowing exactly what I was doing, but with weights I not only know what I am doing, but the square root of what I am doing. I am dealing with very diminished increments. I will add another tolerably heavy weight; it goes up about a tone. So you see small errors in the weight are very easily neglected, and the net result is more likely to be true, in the ratio of the square root to the natural number.

Now single observers have often gone before their time, and I think there are few one could name who have been less appreciated during their lifetime than the next I have to mention, I mean Mr. Griesbach. Through the kindness of Mr. Chappell, we have a splendid exhibition of Mr. Griesbach's instruments here. Griesbach not only measured in days when measurement was little understood, but he reproduced graphically on a sheet of paper the measurements that he made. Here is the original instrument with which he did it. He had a heavy string, and there I most heartily agree with him, not only because the string was heavy and gave slow vibrations which could be counted, but because I think we rather neglect the lower tones. The string he used, a heavy double-bass string, was the one on which these mathematical relations could be most easily determined; at any rate, easy or difficult, he did deter-

mine them. He had a rotating bow of horsehair running round two pulleys; with this he could bring the string into perpetual and steady vibration, and could estimate the curves formed by it. This, I believe, was the earliest instrument of accuracy and value since the time, perhaps, of Pythagoras, but certainly of Galileo. We have a simpler plan in these later days of keeping up the vibration of any string whose natural tendency is to evanesce and fade away, by the use of galvanism. It is almost too simple to require demonstration, but this is an instrument provided for the purpose. Here is a common electric bell, an aluminium bar attached to it, and a cork covered with leather. If I merely set it going with the finger, it will produce only a momentary vibration in the string, but by the galvanic current we can produce permanent vibration, and so be able to measure the tone which it gives.

I am anxious as time is running on, to proceed to another application of the same principle. We can excite strings by striking; we can produce approximate permanence in their sounds by bowing; there is a third mode of exciting vibration, which has only lately attracted its proper attention, but that I leave to a person infinitely more competent to explain it than myself, I mean Mr. Baillie Hamilton. He has shown, and I think very brilliantly shown, that by the impact of air upon a vibrating string, whether through the medium of a reed or by means directly of the air itself, it can be put into permanent vibration, and can be used to produce an exceedingly smooth, powerful, and, if I may use the word, musical quality of tone. Monochords can be bowed, or we can do as we do on the fiddle, play pizzicato, a well-known thing with musicians; and so produce approximate permanence of tone. I have to point more closely to what you get when you pluck, bow, or otherwise excite the string. You get numerous upper partial tones, and those upper partial tones depend firstly on the nature of the stroke. That is very easily shown. I am here quoting from Helmholtz. Take your nail and you get a very nasty sharp sound; take the pad of your finger and you get a much sweeter sound, or you may strike the string with a hammer as is done

commonly in pianos and instruments of that kind. But the tone depends directly on the density, rigidity, and elasticity of the strings. It has been found out by pianoforte makers that if you strike the strings in a particular place, about one-eighth or one ninth from the extremity, you exclude certain disagreeable harmonics and get a much sweeter quality.

Now these peculiar conditions of vibration produced in a string have been exceedingly well examined and illustrated by Helmholtz. He has formed what I cannot demonstrate here, because it is a thing to be seen individually; a vibration microscope. It is not difficult to understand. If you take the object glass of your usual microscope, and instead of fixing it rigidly, fix it to one end of a tuning fork, you obviously give it vibration which, although in a circular arc, is in so short a circular arc, that it may be taken within small limits as being in a right line—a line at right angles to the prong of the fork. You then apply this to a string, the vibrations of which are in a transverse direction, and you produce a compound figure formed of two small straight lines, and, therefore, practically a composition of rectangular vibrations. This rectangular vibration has been produced in infinite ways with much ingenuity. In this particular exhibition there is Messrs. Tisley and Spiller's apparatus; there is the old apparatus of Sir Charles Wheatstone; there is an apparatus by Professor Donkin, of Oxford, and here is Mr. Pichler's apparatus. Mr. Pichler combines two resonating reeds vibrating in the same period, or not in the same period, according as you choose, producing sound of the same pitch or not. To each is attached a mirror illuminated by means of the oxyhydrogen light. On the screen you will see figures more or less homogeneous and regular, according as they are in simple ratios to one another or not. While this is getting ready I will show you another simple experiment, due to Helmholtz, which does not require so much the instrumental aid. I wish to show that in an instrument depending on consonance and resonance, the resounding body which is affixed to the string is consonant in a definite period to the string itself. Helmholtz has found that if you blow into the body of a good

fiddle, and I think this I may honestly say, although I am the fortunate possessor of it, is a good violin, you get a certain note; if you blow into the body of a tenor, which is meant to speak a little lower, you get a somewhat lower note. There is a wind chest here, and I propose to show you the different notes in which they speak. Any musician, I think, will be able to recognise it as a more or less musical note, and Helmholtz's experiments show that the tenor speaks with a lower note. On trying the two I think there is no question about it. Helmholtz says the tone of the fiddle is one whole note above the tone of the viola, and you will agree with him that it is so.

Now I will ask you to look at this image on the screen. There is first a single linear vibration, and then a vibration in the transverse direction. Then you have them together, and see two ellipses rapidly evolving. Here you get beats corresponding to difference of vibration. After that beautiful demonstration which combines the eye and the ear in one, I may also show you some charming figures, which Messrs. Tisley and Spiller have succeeded in producing, but I know no process which simultaneously shows to the eye and the ear the coincidence and discord of sound vibrations like that of Mr, Pichler.

We have to think next of vibration of rods, bars, and tuning forks.

These are exceedingly little used in what is termed artistic music. When a bar is struck it is apt to give extremely high upper partial tones. When it is supported at two points they are somewhat less, and we get the ordinary harmonicon, a dreadful thing, although certainly used by no less a writer than Mozart. I have here a bar of steel, a somewhat heavy one, and if I hold it in the middle and strike it with a hammer, we shall obtain the fundamental tone and some exceedingly high persistent harmonics, many octaves above the fundamental note of the bar. I do not think this simple experiment is quite appreciated, namely, the use of a bar of highly elastic material (glass will do, but steel is better) to show the enormously high harmonics thus produced; the use of a bar like this will explain a great deal that many persons find difficult in Helmholtz's beautiful demonstra-

tions. The upper partial tones are many octaves above the foundation note which is given by striking the bar across; in fact, I know of no better plan of producing extremely high notes. Glass, stone, even wood, almost anything will produce a tone. Here are a number of bits of wood that you may throw on the floor, and as each is thrown down it produces its own tone very distinctly, but we can go beyond this. We may actuate a piece of steel, or metal, or glass, or wood, by means of a bow, and then we attain that curious instrument, shown in the Exhibition, belonging to Mr. Carl Engel, termed the "nail fiddle." I do not suppose the nail fiddle will ever be competent to play Beethoven's symphonies, but it is a very curious fact in the history of musical sounds.

By sticking a few tenpenny nails into a circular piece of wood and bowing them with a fiddle bow, you can produce a complete series of sounds; or you may attach each piece of metal by a spring, thus making it into a musical box, some of which produce harmonies very far from being contemptible; or you may excite it partially by wind, and partially by the finger as in the old jew's-harp. This course is carried to the highest limit, where I must for the moment leave it, in the harmonium.

I will now take you back to tuning forks which form a great part of the experimenter's apparatus. They may be looked upon simply as double vibrating rods. This rod which I have here, I have to keep vibrating by my own power of excitation. I add another, which balances those vibrations by vibrations in the opposite direction, and we can dispense with the need of a firm fixture. They, like the rods, have extremely high harmonic secondary tones, and they are amongst the instruments whose use has extended from sound to other branches of physics. Therein they deserve the greatest honour. Here is a magnificent tuning-fork chronograph, lent us by the French, in which it has been utilised for measuring small intervals of time. You are perfectly certain when you use this fork that it is vibrating at 120 or 250 times in the second, or whatever it may be, and it will not alter from that, therefore you are entirely independent of

any mechanical errors in your recording apparatus. And see what an enormous advantage you gain here from the humble and despised department of sound. You have a fixed measure which will record its measurement upon your blackened cylinder, without any fear of the fly-wheel moving too fast, or that the weight may be too heavy; in fact, all those difficulties which involve in them the instrumental troubles of making experiments.

Alone, of course, tuning forks have a very feeble tone, but when combined with a resonant chamber of the proper size, they give a very powerful note. If I take an ordinary tuning fork you will hardly hear it, but when I place it close to a resonant chamber, there is no mistake about it. They can also be combined with a string. The weak point of tuning forks is the very evanescent character of their sound. That can partly be got over by bowing them, but they require to be bowed very vigorously, and the experiment is not very successful. A better plan is to galvanise them, and for that purpose there is an instrument here. Here is a tuning fork having on the upper prong a wire which dips into a mercury vessel; whenever it comes down it closes the circuit, and thereby, this electromagnet at the side is brought into action and tends to pull it open. As it pulls it open, it draws away the wire; the contact ceases to be made; but by its own vibrating power the fork falls back again, makes contact and gets another pull, so that the tuning fork is always excited synchronously with its own vibration independently of other excitation, and thus is a very good means of transferring vibration even to long distances from itself. This has been utilised extremely well by Helmholtz. I cannot show you his mechanical arrangement for reproducing sounds; indeed I can only allude to it. He proves that the vowel sounds differ from one another by the presence of extremely high harmonics, and he combines together a series of tuning forks so that those high harmonics which accompany each vowel are produced by the mechanical combination. How startling the effect is I can speak from experience, the starting out of the peculiar vowel sounds *a*, *e*, *i*, *o*, and *u*, is something unexpected

and almost painful. We are so accustomed to hear vowel sounds only produced by human beings that, when a musical instrument produces them, we almost jump back in horror; we seem to have some Frankenstein speaking before us of whose presence we have never before been conscious. However, this intermittent contact has an ulterior value, and in its ulterior form I hope to show it you to-night.

I will go on now to sirens. Sirens are utterly unknown to music. I do not think anybody living ever played a tune on the siren, but they have contributed, on the other hand, more to the ascertaining of the fundamental laws of sound, than any other instrument. They have not only contributed to these valuable purposes, but they have contributed to other sciences. It was through the means of a siren that Foucault first obtained his determination of the velocity of light. The siren is, in its simple form, a perforated rotating disc in front of one or more holes delivering air; and we have three forms of it, but I only purpose to illustrate the first and the last. The first form of siren is what is termed Seebeck's. It is a perforated disc of cardboard, which I can put in connection with a little air chest, and rotate it. When the rotation comes up to a moderate amount, you hear a sound, depending on the number of holes. Cagniard de la Tour improved the instrument by making all the holes blow on one another; still the sound is small. Therefore, I may at once begin to speak of Dove's siren as reduplicated by Helmholtz. Here is the original siren, and here is that which was used by Helmholtz, the principal instrument in determining the rapidity of the vibration of sound. I have in it two sirens so adjusted that I can use either or both. When put in motion, you first hear a growl which gradually rises in intensity until we get to the character of a musical note. With the same siren we can get several notes, from a series of 9, 12, 15, and 16 holes. These you will easily realise by means of their proper sound. And I have the same thing on the lower disc, only with somewhat different numbers. Time would fail to go into all the applications of this very important instrument. I can produce

on it the common chord; I can also produce not only harmony but every variety of discord.

The reinforcement of sound is almost as important as its production. Not only in sound, but in every vibrational motion, reinforcement is of importance. We all know the child's swing; every time it comes to you, if you give it a push it receives a greater impulse. You find the same result under many other conditions. In bell-ringing, for instance. I remember in olden days when the bells were rung in Magdalen Tower, on the 1st of May, if you were standing on the top with the whole peal of twelve bells ringing together, the tower swung about like a ship in a gale. It would not do that under any force impressed upon it, except a vibratory force. Generally speaking all sounding bodies also reinforce, and some of them have been well termed in the Handbook "distributors" by Professor Clerk Maxwell. There is again a condition under which bodies will single out particular sounds, and here is the most important branch of the science of sound. Helmholtz has studied this with great care. If you take an ordinary violin and hold it near your ear, with all these miscellaneous sounds from the siren going on, you will find the violin speaks to some sounds and not to others; it likes some, and dislikes others, and probably it is very right; you do the same yourself. It reinforces certain sounds and leaves out certain others. In a pianoforte the same thing occurs, if you take up the pedal and let the strings speak, they will reinforce any sound you sing or speak into it with great vigour; but perhaps nothing is more remarkable than taking simply the top of an ordinary hat. If you go to the opera into the gallery, where the sound comes up in great purity, and simply put your finger on the top of your hat, you will find the difference between consonant and dissonant sounds; when a powerful consonant sound comes from the orchestra, the top of your hat will vibrate so powerfully as almost to throw your finger off. It has, in fact, a preference for certain sounds; it has a view of its own, and that view it very properly reinforces. This has been worked out by Helmholtz exceedingly well in what are termed resonators. For the external membranes which he

originally used he substituted later the tympanum of the ear. By taking tubes of various dimensions you can reinforce any sound. I have a resonator here which speaks at the pitch of F, and you will find if my voice goes as high as F, the sound is powerfully reinforced when you have this resonator applied to your ear. I think it is worth while distributing some of these amongst the audience; it will be found that you will hear a sound reinforced by each resonator when another is not affected. The inflection of ordinary speech, of course, passes through a number of consecutive notes without any distinct interval, and the resonator will pick out the one to which it resounds. We have two or three very good series indeed of these resonators in the Exhibition.

As to the real objective existence of these partial tones, thus picked out by reinforcement, there has been considerable doubt amongst musicians, but I think Helmholtz speaks with great force and conclusiveness when he says they really are existent and cannot be excited out of nothing.

Lastly, I have to speak of a new and remarkable kind of distribution in the form of electricity, applied to musical sound. It has long been known that iron when magnetised gives a peculiar clink; the molecular constitution alters; it lengthens, as may be proved in other ways, and this molecular alteration is shown in the production of a minute sound. If the molecular constitution be altered regularly by a series of vibrations bearing a definite ratio to one another, this clinking becomes a musical note; I have here an ordinary harmonium reed to which I have attached a wire of aluminium, and this is connected with a battery. If I now blow this harmonium reed by means of wind, and thus make a regularly intermitting circuit, those who are within range of the resonator will hear it clink, though it is not audible at a great distance. This, then, the original electric idea, has been developed on a very remarkable instrument with which I will conclude. It has been developed in two forms, first by Reuss, a German, and it has been carried very much further by one of our Transatlantic brethren, Mr. Elisha Gray, of Chicago. Reuss's instrument was described in the

'Telegraphic Journal' a month or two ago, and Gray's instrument, by the kindness of Mr. Latimer Clark, I am able to present to you this evening in working order. I have also a letter of Mr. Gray's own, in which he explains his own instrument. He says, "I have been very busy experimenting since I came West and have developed many facts bearing on the transmission of musical tones. The most important is this. I have determined the fact that a number of tones differing in pitch may be sent through the same wire at the same time, and analysed at the receiving end, so that the different notes are heard, each on its own instrument, distinctly and independently; by using a common Morse signal at the sending end, any given note can be read on the receiving instrument corresponding in pitch, and no other. Then the same thing can be going on at the same time with all the others, so that instead of duplex or quadruplex we have in this multiplex, limited only by the number of perfect chords in the seven octaves, about twenty times in all." First I should like to show you Reuss's instrument, in which the voice is telegraphed from end to end of a conducting wire. I do not pretend to be a great singer myself, still less am I accustomed to sing to telephones, which are doubtful in their interpretation, but I think I can show those who will kindly go to the other end of the room, where the receiving instrument is placed, that my voice will be transmitted by means of the membrane, and that I shall be able to transmit the four notes of the chord from four tuning forks, on to the receiving instrument according to Mr. Elisha Gray's practice. At the same time let me call your attention to the fact that this is a most remarkable transformation of energy. We here change sound, vibration of which we know everything, of which, perhaps, there is more known than of any other form of vibration, into a form of energy of which we know comparatively little. No doubt electricity is a molecular force, and probably a vibratory force; I think the researches of Mr. Clerk Maxwell show that we shall very soon tie it on to sound, light, and the other accepted forms of vibration. What we have done in this curious instrument is that we have produced the results empirically, and by way of anticipating the subsequent

demonstration of their theoretical union. We can carry sound vibration, transmuted into electricity, along a wire, and at the end we can reproduce it, certainly not a little damaged; for, as a musician, I do not think the receiving instrument speaks quite so good a tone as I sing; still it is the same note, and the telephone reproduces it at the distant end of the wire as musical vibration. We have re-constituted the *Notum per ignotius*.

The CHAIRMAN, in the name of the audience, thanked Dr. Stone for his interesting Lecture.

GALVANIC TIME SIGNALS.

By C. V. Walker, Esq., F.R.S., F.R.A.S., etc., Hon. Member Horological Institute, President Soc. Telegraph Engineers.

17th July, 1876.

James Glaisher, Esq., F.R.S., etc., in the Chair.

The Chairman: Ladies and Gentlemen,—The collection of loan apparatus in this building cannot fail to be productive of a great deal of good, and I do not know of any one subject which is more suggestive of good than that upon which the Lecturer is to speak to-night. It is now nearly thirty years since I had a certain conversation with Mr. Walker, who will lecture to us presently, and that conversation was the origin of the time-signals that are now sent all over the world. I commenced by saying that this loan collection would be productive of great good, and I had in my mind the consequences that had flowed from that conversation; and when you consider how many gentlemen will be brought together here, and will be talking over the different instruments, it is impossible to say what seed will be sown, and what good may not be gathered and garnered in many a year to come from these conversations, which at the present moment may seem to be of very slight importance. The subject which the Lecturer will explain, is the sending of galvanic signals from the Greenwich clock, hour by hour, simply by means of certain little apparatus which make contacts. I merely mention this to show how simple the method is; and without further remark I will introduce to you Mr. Walker, F.R.S., who was at the birth of this system,

and has watched its growth from then till now. He is thoroughly conversant with every part of it, and knows its complete history; and, probably, the fact of his having to deliver this Lecture to-night, may save that history from being lost, as I am in hopes that he will give us an account of the birth and early history of galvanic time signalling. I will now introduce him to you, and I am sure you will be very much interested by what he has to tell you.

MR. WALKER: One word before I commence this lecture, in order that you may not feel alarmed at 9 o'clock, should you hear the time gun fired. It should be fired by Greenwich signal; but I cannot quite answer, however, for its firing; because it so happens that at this particular moment the Astronomer Royal is in communication with Vienna in regard to longitude observations in connection with the new Observatory there. Should you hear a sudden sound at nine o'clock, you will only know that it is time for me to conclude; and you will not be alarmed at it.

Our subject is "Galvanic Time Signals." Three words; I shall have but little to say upon the first two. The last word "signal" will form the substance of the address which it will be my duty to deliver before you this evening. Galvanism, time, and signals will be so interwoven each with the other, that as we proceed we shall, to some extent, lose sight of their individuality.

I make no attempt to call what I have to say, a "Lecture." It will be almost barren of experiment or illustration. It will be a mere description, as free as I can manage to make it of all technicality and of all *ad captandum* remarks.

With the Astronomer Royal, treading indeed in his very footsteps, I cling this evening to the good old word Galvanism, and to the memory of Galvani, who was the first to put on record the effect of the action of electricity upon the lower limbs of frogs. This led up to Volta's investigations, which have given to us an instrument, the value of which in scientific research is—shall I say—more than equalled by the important service it has rendered in applied science, I mean the Voltaic battery. In its practical form it consists of two metals, and one, sometimes two, liquids. Carbon is often substituted for one of the metals. Metals and liquids being almost without number, you can well

imagine that—their combinations being equally unlimited—the varieties of Voltaic batteries must be great; and so they are. I shall call your attention to two specimens only, one of the simplest possible character, the other somewhat more complex, but each equally valuable in its proper place.

In both these batteries *zinc* is one metal, and *carbon* takes the place of the other. In one case the carbon is coated with the fine powder of *platinum;* in the other it is surrounded by peroxide of *manganese.* In the former case the liquid is sulphuric acid and water; in the latter, sal-ammoniac dissolved in water. I need hardly tell you that these special arrangements were entirely unknown to Volta. They are the result of inquiry and experience carried on almost to the present day. The quest has been after an instrument that would stand *ready* to give out, when required, a fair supply of electricity, and would not consume itself when *waiting* for work.

The Plat-graph arrangement dates back to 1857, or nineteen years ago. The other, which is due to M. Leclanché, and known under his name, was practically introduced into England about eight years ago, but was favourably known on the Continent for some time previous.

The Plat-graph is used by the Astronomer Royal for time-signals. For myself, I have in use, for general purposes, over 9000 Plat-graphs and 3000 Leclanchés. I have instances of the former doing their ordinary daily telegraph work *entirely* untouched for 12, 14, 15, 17 months; of the latter for 14, 18, 25, 26, 29 months. Were it not for *evaporation* the periods would have been much prolonged.

Volta's name is not quite so often on one's lips as Galvani's; not that the name of Volta has been displaced by any rival name. There is none. For his discoveries have been taken up in all directions. Nature has been literally ransacked for materials. New *combinations* have followed each other, and new names remain on record. We have Daniell's battery, Smee's, Grove's, Bunsen's, Marié-Davy's, Plat-graph, Leclanché's, De la Rue's, etc., all voltaic batteries, with the name of Volta left out, just, indeed, as the word "battery" itself is left out in common parlance, and

we speak of a Daniell, a Smee, a Leclanché,—"Voltaic battery" being understood. We have his name in the Voltameter, an instrument used for measuring the gases released in the decomposition of water, etc.

But here is an instrument of another kind, intimately connected with the subject of this evening, which has made Galvani's name a very household word to us; and has so grafted it into the vocabulary of Science—pure as well as applied—that it is not likely ever to be lost to us. The instrument is called a Galvanometer, a measurer of galvanism or galvanic electricity; in all cases a qualitative measurer, and under special arrangements a quantitative. It consists essentially of a piece of wire and a compass needle. If a wire and a magnetised needle are placed parallel, each with the other, and electricity is sent along the wire, the needle will move; and if the electricity is powerful enough, and the needle light enough, the latter will place itself at right angles to the wire. It turns to the right if the electricity is moving in one direction; to the left if moving in the other.

But it is not a usual thing to find electricity in a single wire powerful enough to move a needle to right angles; it moves it to a small angle only; two wires affect the needle more powerfully than one; three wires still more; and so on. Speaking generally, the effect is increased according as the wires in action are multiplied. For which reason the Galvanometer has been also called a Multiplier. Practically the instrument is not constructed with a multiplicity of separate wires. A sufficient length of one and the self-same wire is used. It is covered with silk or cotton, and wound on a frame around the needle and clear of it, so that the same current of electricity in passing along from end to end of this wire presents itself many times to the needle.

Well, now, if you or I desire to have accurate time, and are not able ourselves to obtain it by direct observation, we must associate ourselves with some one who is able. He must be provided with a galvanic battery, and we must possess a galvanometer; and a wire must be erected in a proper manner from him to us, no matter what the distance. We shall arrange with him, when it may be most convenient to all parties concerned, for true time to

be sent by him and received by us. With the means at his disposal he will find out when the selected second of the selected minute of the selected hour has arrived, and will send some galvanism from his battery along the wire to our galvanometer. We shall be on the look out, and will see our needle move to the time. Electricity travels so fast that no account needs be taken of time lost on the road.

Before passing on to the arrangements that are in use for finding true time and sending it to other people, I must just say one word in explanation of another instrument that is used for time-signals, equally with, but not so generally as, the galvanometer. I mean the Electro-magnet. An electro-magnet consists essentially of a bar of soft iron, straight or otherwise, within-side a coil of wire, covered with silk, cotton, or otherwise. In the absence of electricity, the iron is merely iron; in the presence of electricity, that is of a current of electricity circulating in the wire, the iron becomes a magnet; and by proper arrangement may become, if required, a very powerful magnet. When the flow of electricity is interrupted, the magnetism leaves the iron, to return only when the electricity returns. Magnets attract iron: they attract or repel other magnets, according as different or like poles are presented each to the other. The mechanical force thus obtained is turned to useful account, in driving, controlling or regulating clocks, and in dropping Time-balls.

Time. When I look round upon the audience before me, I can hardly think that there is one person present, old or young, who does not either openly or in his heart desire *more* of something; more *income*, more *health*, more *knowledge*, more *friends*, more *position*, more *possessions*, and so on. And it is perfectly possible for any one of you to possess at this very moment more of some of the things I have named than he thinks he owns. Taking the first and the last in the list, *income* and *possessions*: Who knows but that there is some one in this room, who is actually richer in this direction at this moment, than he was when he entered it half an hour ago? A bequest or a gift may have done the work; and the good news has not yet reached him.

But things are different in regard to *time*. It is perfectly

impossible for any one of us to have any more time than we now possess. In common parlance we are apt to say, give me a little more time and I will do this, that, or the other. But this is not what I now mean by more time. I mean that until the present moment is gone from us absolutely, never to be regained, we cannot have another; we cannot be possessed of two seconds of time at once.

It is the object then of galvanic time-signals to give you, not the mere hour or minute, but the absolute second or moment really of time then or now in your possession; to identify, in fact, and give you the name of that particular moment.

It is my purpose in the first instance to occupy your time with the earliest history of galvanic time signals. Mr. Glaisher has truly said to you that but for the present opportunity or some opportunities like this, it really might have been lost; in fact, although having taken so very active a part and so very early a part in the initiation of time signals, even with myself there are two or three missing links in the very early days. Nor am I in very bad company in that respect; for the Astronomer Royal, who co-operated with me and I with him, made this remark in 1865, "I can hardly say how the time-signal system came to be first proposed. It was somehow, partly in conversation and partly in other ways; how I cannot exactly say, but to Mr. C. V. Walker, Mr. Edwin Clark, Mr. Latimer Clark, and afterwards Mr. C. F. Varley is the existence of the system mainly due." This remark was made by the Astronomer Royal at the conclusion of the Lecture delivered by Mr. Ellis, of the Royal Observatory, before the Horological Institute on February 24. Mr. Ellis had taken a very active part in the matter; it was his branch of work in the Royal Observatory.

At the commencement of his lecture Mr. Ellis says, "From the very first establishment of the various telegraphic systems in England, the Astronomer Royal had always kept in view the desirability of connecting the Royal Observatory with those systems for various purposes, among others for the distribution of Greenwich time. About the year 1849, he came into correspondence with Mr. C. V. Walker, Telegraphic Engineer of the South Eastern Railway

Company, who on his part was equally desirous to place the South Eastern telegraphic system in communication with the Observatory; and the necessity for other communications having also arisen, the Astronomer Royal obtained the permission of the Directors of the South Eastern Railway Company (on the representation of Mr. C. V. Walker) to carry wires on the poles of their railway to London Bridge."

In referring to letters and other documents, the first link that I find in the history is an extract from my diary, dated 13th April, 1849.—"Visited Royal Observatory, and saw Mr. Glaisher on the plan for meteorological observations." And this must be the date of the conversation to which the Chairman has referred. —On May 10th, 1849, I received a letter from my friend, Mr. Glaisher, to this effect, "I also wish to talk with you about the laying down of a wire from the Observatory to the Lewisham Station." Then on the 23rd of that month of May, the first letter from the Astronomer Royal reached me, from which I will make a few extracts. In considering the probable use of a galvanic connection of the Royal Observatory with the telegraphic system of the South Eastern Railway the following remarks occur,—"It is not likely that such a connection would be used for *messages*, other than simple signals, for these two reasons:

"1. That we have not any person constantly in attendance.

"2. That my assistant would not learn either to read or to deliver the messages with facility. Indeed it would be an easier thing for us to send a message to Lewisham than to work it here. The use of the telegraphic connection would be mainly, perhaps solely, in the transmission of time-signals, no unimportant object however. Still if it was restricted to the S.E. district, its utility for this purpose would be exceedingly limited in comparison with the utility which it might have. This deserves consideration for this reason:

"1st. The abstract question of general utility, which I am sure will have weight with yourself and with the Directors of the Company.

"2nd. The claim which it would equitably create for defrayment of part of the expense by the Government."

"The instances in which I have practically wanted to communicate time by signal are,—To and from Liverpool and to and from two clocks in London (the Royal Exchange and the Houses of Parliament clocks). I have from the first indicated as a desirable thing, that every stroke of the latter in striking the hours at least once in the day (and the same applies to the former) should be observable at the R. Observatory. But generally speaking in a country like this where so many ships rely on chronometers for navigation, where in every transaction of business the importance of punctuality is thoroughly understood, it is very desirable to have in every large place means of knowing the time accurately. This consideration renders it very much to be wished that any line from Greenwich should be placed in galvanic connection with the lines North of the Thames."

Then he goes on to say, "I should be very glad to discuss with you in face of the instruments the mechanical means for doing this. I believe that a clock may be set up at Greenwich, which will do all our part in giving time automatically." I immediately wrote in reply, —"I thank you for the remarks contained in your letter of yesterday, which will enable me to have a clearer view of the general question of the utility of a telegraph wire between the R. Observatory and the S. E. R. Company's system. You will find me ready cordially to co-operate with you in this important matter. I will take an early opportunity of conferring with our Directors, now that I am able to give them some particulars. I shall be very glad to enter with you into the practical features; and will write again," and so on. There you see the ice was broken.

The Astronomer Royal writes to me on the following day, 24th May, and sends me a scheme proposed by him "for the transmission of Greenwich time by galvanic signal to every part of the kingdom in which there is galvanic telegraph from London." I need hardly weary you by reading the whole scheme; naturally when we discussed it together, it ripened under our hands. The substance of the propositions, that were in due course realised, is:—That a distinct hour or other time should be adopted for each line to receive its signal;—that a clock should be erected at the

Royal Observatory, which should be adjusted daily so as never to be more than a fraction of a second in error;—that messages on a telegraph wire should be suspended, for a time-signal to pass, for not more than one minute;—that, if a time signal failed when due, a look-out should be kept for the next;—that clerks should keep their wires clear of messages when the signal was due;—that the Observatory wires along the South Eastern Railway should be extended from the London Terminus of that line onward to some central station;—and that, "in any case, however, the power of giving time to England generally must depend entirely on the co-operation of the S. E. Company." I wrote to Mr. (now Sir George) Airy very shortly afterwards, that is, on the 11th June, 1849, "I have conferred with the Directors respecting the wires between the Observatory and Lewisham Station. They expressed their readiness to promote scientific objects, but that they had not money for this purpose." But you will see as I proceed that they have furnished what in this instance is of far higher value than mere money, namely, money's worth. I have before me a volume of the annual Reports made by the Astronomer Royal on the first Saturday in June, at the Visitation of the Royal Observatory by the authorities; and here we find the first official note on the subject. The history of the Observatory during the past year is given up to the lunation that had closed last before the Visitation-day. In the Report read on June 2nd, 1849, time-signals are thus introduced:—"Another change will depend on the use of galvanism; and, as a probable instance of the application of this agent, I may mention that, although no positive step has hitherto been taken, I fully expect in no long time to make the going of all the clocks in the Observatory depend on one original regulator. The same means will probably be employed to increase the general utility of the Observatory, by the extensive dissemination throughout the kingdom of accurate time-signals, moved by an original clock at the Royal Observatory; and I have already entered into correspondence with the authorities of the South Eastern Railway (whose line of galvanic communication will shortly pass within nine furlongs of the Observatory), in reference to this subject." In every annual report which the Astronomer Royal has made—

the volume before me extends to the year 1868; and there are the eight Reports since delivered, up to the one delivered last month—he never fails to continue the history of the progress of time and electricity; and he also never fails to recognise the services of those gentlemen, who took part with him in organizing the system. He says on June 1st, 1850, "I alluded in my last Report to the possible galvanic connection of the different clocks of the Observatory, so as to make the motion of every clock depend on the motion of one: . . . The second point is, the connection of the Observatory with the galvanic telegraph of the South Eastern Railway, and with other lines of galvanic wire with which that telegraph communicates. No arrangement is yet effected for this purpose, but I continue to keep my attention on it, even with greater interest than formerly. I had then in mind only the connection of this Observatory with different parts of the great British Island: but I now think it possible that our communications may be extended far beyond its shores. The promoters of the Submarine Telegraph are very confident of the practicability of completing a galvanic connection between England and France: and I now begin to think it more than possible that, within a few years, observations at Paris and Brussels may be registered on the recording surfaces at Greenwich, and *vice versâ*." These dates are very interesting. Beginning from the conversation with Mr. Glaisher, there was, as you have heard, a great deal of correspondence and a great deal of arrangement. In 1851, on August the 9th, I began erecting two wires from Lewisham to London. One of the wires—so it was then proposed—was for the Houses of Parliament and the Royal Exchange clocks, and the other for general purposes in London and throughout the kingdom. On the 19th March, 1852, those two wires were completed; and, after completing them, the Astronomer Royal, on May 17th, 1853, ordered two more wires, making four; and by the end of the year 1853 these four wires were completed from Lewisham to London, and from the Royal Observatory to Lewisham. But to return to the Annual Reports of the Astronomer Royal:—that read on June 2nd, 1851, makes no reference to our subject. The

following extracts are from the Report read on June 5th, 1852 :— "In the last autumn, [1851, Sep. 28] the Submarine Telegraph between the South Foreland and Sangatte, in France, was successfully completed, and in a very short time afterwards I received from some of the active members of the Institute of France an earnest request that advantage might he taken of this event for connecting the Observatories of Paris and Greenwich. I proceeded without delay to negotiate with the great commercial bodies (the Electric Telegraph Company and the South Eastern Railway Company) whose assistance was necessary, and whose rights might be affected by such a connection; and by them my overtures were received in the most liberal spirit. To these bodies generally, and to their Superintendents of Telegraph in particular (Charles V. Walker, Esq., for the South Eastern Railway, and Edwin Clark, Esq., for the Electric Telegraph Company), my most cordial thanks are due, for their adoption of my proposals in all their fulness, and for their hearty co-operation in every part of the work." And he then mentions how that four wires had been laid underground from the Observatory to the Railway Station at Lewisham, and thence along the railway to London Bridge Terminus, "where the connections will be made, either with the long Dover wires communicating with the Continent, or with the wires that extend to the Central Telegraph Station." Meanwhile on August 5th, 1852—which is a memorable date—a CHRONO-TREPETER was mounted for time-signalling purposes in the turret clock at London Bridge Station; and at 4 P.M. on that date the first time-signal, that was ever passed out of the Royal Observatory, reached London in my presence; and four days after in the presence of myself and Dr. O'Shaughnessy (Brooke), F.R.S., whose name is well known in the scientific world. August 9th, 1852, is the date of the first time-signal that went to Dover direct from Greenwich.

From this 9th of August time-signals began to be sent regularly from Greenwich. On the 1st November, 1852, the system of time-signals was thoroughly established throughout the South Eastern Railway; they were sent direct from Greenwich to Dover and the chief stations; and by hand from the junctions to the Branch lines.

I will now describe the original Chrono-trepeter, which is before

you; and which is a train of wheels with some contact-springs. It was in gear with a 60-minute wheel of the clock; and the adjustments inside were set in such a way that a wire from Greenwich at one o'clock, for instance, should be properly connected with a wire that led to Dover, and kept there for about a couple of minutes, so that when the signal left Greenwich it passed uninterruptedly to Dover. That is the principle upon which this instrument had to act, the operation being simply that of bringing two springs together at the proper time.

On June 5th, 1852, Shepherd's electric clock had been erected at the Royal Observatory, and was fairly started to do all the necessary work of connecting the proper Observatory signal wires with the railway wires. I will describe the electric clock presently. The Astronomer Royal and myself tried what was then a very bold experiment. We connected a companion electric clock on the 16th September, 1852, in London, at the London Bridge Station, with, and to be driven by, the clock at the Royal Observatory. The signals second by second made by the Greenwich clock moved the clock in London.

In 1853, on February the 3rd and the 10th, the Astronomer Royal came to London Bridge Telegraph Office, and tested the needle signals from Greenwich, preparatory to making experiments on transits for longitude. On the 10th February, 1853, is the first time I see Deal mentioned in my diary; and on the 7th March I first find the dropping of the ball at Deal mentioned.

The following are from the Report read June 4th, 1853:—"At the date of my last Report, the principal part of the work for carrying two wires to London Bridge (four having been laid in those parts in which the wires pass underground), was by the kindness of the South Eastern Railway Company and the Electric Telegraph Company, completed, but the wires were not so far connected as to be brought into use. Shortly after that time they were brought into daily use." Further on we read:—"The galvanic apparatus for sending hourly signals to London; the sympathetic dial at the entrance gate; the sympathetic clocks in the chronometer-room, computing-room, and dwelling-house; and a sympathetic clock at the South Eastern Railway Terminus, are all complete and in constant use.

"In the employment of the galvanic wires in the Royal Observatory for the several purposes of making registers in the American manner, dropping our time-ball, sending hourly signals to London, dropping the ball in the Strand, passing occasional signals to or from London and stations beyond London, and passing occasional signals to or from Paris, a variety of communications of wires is required." Further on we read :—

"At the time of the Visitors' last meeting, a Normal clock had been erected by Mr. Shepherd, furnished with a small apparatus suggested by myself (an auxiliary pendulum, which can be made very long or very short, and can in either state be connected with the clock-pendulum), by means of which the indications of the clock can be increased or diminished by any required quantity above $0^s.01$. The error of this clock being ascertained every day, by means of another clock close to its side, which has been compared with the Transit-clock. There is no difficulty (with the use of the auxiliary apparatus above mentioned) in making it sensibly correct. . . .

"The same Normal Clock maintains in sympathetic movement the large clock at the entrance gate, two other clocks in the Observatory, and a clock at the London Bridge Terminus of the South Eastern Railway (first tried (p. 12) with the assistance of C. V. Walker, Esq., as an experiment, but now to be used for automatically making and unmaking certain connections of our galvanic wires). . . .

"I have the satisfaction of stating to the Visitors that the Lords Commissioners of the Admiralty have decided on the erection of a time-signal ball at Deal, for the use of the shipping in the Downs, to be dropped every day by a galvanic current from the Royal Observatory. The construction of the apparatus is entrusted to me. . . .

"On the nights of May 17 and 18, excellent series of signals were passed backwards and forwards between the Royal Observatory and the Railway Station at Cambridge. No wires have been led to the Cambridge Observatory; and Professor Challis was therefore compelled to carry chronometers, previously compared with the Transit-clock, to the station. . . . At the moment of my writing [May 28th], the observations are not fully reduced. . . .

"I have also made every arrangement with Professor C. Piazz Smyth for the interchange of signals on the night of May 25 and trust to be able to state to the Visitors the result."

In an "addendum" to this Report, under date of June 4th, as the result of 279 signals, the longitude of Cambridge is given as $22^{s}.956$.

On the 17th May, 1853, the Astronomer Royal received the sanction of the Admiralty to erect a time-ball at Deal; on the 20th March, 1854, this time-ball was very nearly ready; and on the 5th of April I rowed out from Deal to H.M.S. St. George, then in the Downs; and from on board that ship saw the ball dropped. It was a trial of the ball—dropping it by hand. On the 9th May the Astronomer Royal and I went to Deal, and the ball was dropped from London Bridge Telegraph Office at 4.13 P.M. for the first time; and in the evening, at 11.10 P.M., the ball was first dropped from the Royal Observatory, in this instance, by hand. On the 23rd May, at 1 P.M., it was properly dropped automatically and for the first time by the clock arrangements of the Royal Observatory, and those at London Bridge.

In the Astronomer Royal's Report, made on June 3rd, 1854, we read of the Deal ball as having been completed. "The ball has now been erected by Messrs. Maudslay and Field, and is an admirable specimen of the workmanship of those celebrated engineers. . . . The automatic changes of wire communications are so arranged that, when the ball at Deal has dropped to its lowest point, it sends a signal to Greenwich to acquaint me, not with the time of the beginning of its fall (which cannot be in error), but with the fact that it has really fallen. The ball has several times been dropped experimentally with perfect success; and some small official and subsidiary arrangements alone are wanting for bringing it into constant use. I can scarcely convey to the Visitors how much I am indebted to the South Eastern Railway Company and the Electric Telegraph Company, and to their principal telegraph officers, Charles V. Walker, Esq., and Latimer Clark, Esq., for the liberality and even the zeal with which they have assisted me in every step of these preparations. Without the cordial aid of Mr. Walker, in particular, it would have been

impossible to complete the work. The best line of wires on the railway has been devoted to this purpose, and the shifting connexions have been modified to diminish the resistance and remove the chances of disturbance as much as possible."

On June 2nd, 1855, the Astronomer Royal reports that "The time-signal ball at Deal was brought into regular use at the beginning of the present year." [Feb. 7]

On June 7th, 1856, he reports, "One of the galvanic clocks in the Post-office Department, Lombard Street, is already placed in connection with the Royal Observatory, and is regulated at noon every day."

And on June 6th, 1857, we read that "The communication with the Post-office clock is remarkable. At 23h. 26m. os. of that clock a signal is given to Greenwich, the comparison of which with our clock acquaints us with the error of the Post-office clock. At oh. om. os. of the Greenwich clock a signal is sent from Greenwich, which mechanically adjusts the Post-office clock. At oh. 26m. os. of the Post-office clock a second signal is given to Greenwich, by which the efficiency of the adjustment is shown."

Time would fail me to follow year by year the progress made withinside the Observatory. Suffice it to make one more extract from the Report read on June 2nd, 1860, of the improved arrangement for bringing the Normal clock to true time. The auxiliary pendulum, adopted in 1853, was abandoned, and in its place was substituted "a bar-magnet, which is carried by the pendulum rod, and is parallel to the pendulum rod. The pole of the bar-magnet swings immediately above the pole of the galvanic coil [without core]," through which a current from a small battery can be sent in one or other direction and for a known time. When so sent that "the force is attractive, the force of gravity on the pendulum is augmented, and the clock is made to gain; when the force is repulsive, the clock is made to lose."

On March 28th, 1853, I delivered a lecture at the London Institution; a wire was led from the street to the lecture-table, and to a clock on the table, which clock was driven by the clock of the Royal Observatory, six miles distant.

Now I told you a short time since that, although the

Directors of the South Eastern Railway had no money to devote to scientific objects, you would see their liberality in another form. The South Eastern Railway Company erected four wires from Lewisham to London (there are three of them now, and they now go from Greenwich to London) at cost price; that is, they charged no profit on the work. The Royal Observatory pay the South Eastern Railway Company 5*s.* a year for maintenance, and 5*s.* a year for way-leave; which is, all told, only 10*d.* per mile of wire per annum. These three time-wires (11m. 35ch. in all on the Railway), are thus appropriated:—No. 1 is used for sending a time-signal every hour for South Eastern Railway purposes. The 1 P.M. signal passes on also from the station at Deal to the tower on the beach that carries the ball; and the hour signals, not required by the Railway, pass on by postal wire to the Horological Institute in Clerkenwell, free as far as the railway is concerned; but at an annual charge of £15 by the Post-office. No. 2 is called the "controlling wire," along which signals pass from the Royal Observatory to London every alternate second, and constrain the pendulums of certain regulators to keep time with the mean-time standard at Greenwich. No. 3 is conceded free of all charge to the Postal Telegraph Department; and is connected at London Bridge with one of their street wires. It is by this wire they receive true time for distribution to their offices and to their clients. Their charges for time-signals are given in the British Postal Guide, and vary from £12 to £32 or more, per annum, according to the accommodation desired. The Railway Company charge cost price for alterations; and 5*s.* a year for the loan of a wire from London Bridge to Deal, for about a couple of minutes each day, and which causes no inconvenience to any one. The Observatory were to give the S. E. R. at least four time-signals a day; but, in fact, they give us any amount of time-signals; they are coming to us all day long. They were to provide and maintain a clock at London Bridge, which they do; or rather we do for them, and save them all trouble. The wires, etc., and the clock were to remain the property of the Royal Observatory; and one wire (No. 3) at London Bridge was to be connected as required by the Astronomer Royal with outside wires. "The wires

are to be used for no purpose whatever except for the transmission of time-signals; and of warning signals connected with time-signals; and for the regulation of clocks belonging either to the Government, the South Eastern Railway, or publick telegraph companies;" and this agreement may be terminated on one year's notice being given by either party to the other, at any time.

I will now attempt to describe the arrangements for sending time-signals, which are so exceedingly simple that if I fail to make you understand the principle—I shall not trouble you with the details—the fault will rest with me and not with you. There is a drawing on the board, which will give you a very simple idea of it. One pole of a battery at the Royal Observatory is connected with the earth, and the wire from the other pole goes into the clock, where it is connected with a spring which we may call *a*. In the clock there are two pairs of springs, *a b* and *c d*. A wire from *b* to *c* connects one pair electrically with the other; and a wire from *d* goes by the time-wire No. 1 to a clock at London Bridge; where in the normal state of things, it is in connection with the Clerkenwell wire, also in the normal state, the circuit is cut both at *a b* and at *c d*; so that no electricity can pass out from the battery. Now the arrangement in the clock is simply this,—that the pair of springs *a b* are pushed together, by a pin on the minute wheel of the clock, at the end of every minute; but, so long as the springs *c d* are still apart, nothing happens. But the springs *c d* are put together at the end of every hour, that is, say at 59 minutes or so, and are held together for two minutes; and when, at the 60th second the two springs *a b* touch, the current goes to the Horological Institute in Clerkenwell and moves their needle; and they receive a signal every hour that we concede to them. We undertook, originally, to give them one signal only per day, but it is more convenient to give them every signal we do not want ourselves; and when we require a signal, the clock detaches the Greenwich wire from them. The S. E. R. take a signal at 8 o'clock in the morning, one at 10, then at 12, at 1, at 3, and at 4, and the Horological Institute have all the other signals day and night. At London Bridge we erected a large electric clock in connection with the Normal clock at the Royal Observa-

tory; and started it on March 2nd, 1855. At the hours when the Railway Company require the signals, a camm on a 24-hour wheel moves forward a set of springs into such a position that a finger on an hour wheel lifts the Greenwich wire away from the Clerkenwell wire and presses it against the spring of another wire, which leads to Dover; it not only presses it against the Dover wire, but moves the Dover wire from where it ought to be at other times, and holds the two wires together for about a couple of minutes, and the signal goes in due course to Dover. At another time of the day, an analogous act occurs and the signal goes to Maidstone. A mere glance at the drawing before you of what we call the Gate of the London Bridge clock, showing the apparatus for lifting the Greenwich wire away from the Horological Institute wire, and putting it on to the Dover or Maidstone wire, would show you that it would not be convenient to attempt to weary you with the details. For conveying the signals to Deal the details are also rather complicated, as you will see by looking at that large diagram behind me. On the diagram are a series of red dots, which show the route from Greenwich to Deal, and three or four red crosses; the red crosses are to show where the wire is broken and put together by means of springs. I have explained to you what is done at the Royal Observatory, and you will see there the red cross. If you will follow the dots and reach London Bridge, you will get to another cross;—the operation there I have already explained.

At Ashford, instead of going on direct to Dover, the dots turn off at the Ramsgate branch, and go along to Minster, and from thence to Deal, into the Ball-tower, where I show a rough sketch of the ball. There used to be a clock at Ashford Station that did precisely the same sort of work as that I have already described. It removed the London wire from Dover and placed it upon the Deal wire; and this it did daily just before one o'clock, and held it there for a couple of minutes, until the signal passed to Deal and dropped the ball. There was something special about the clock at Ashford, to which I shall refer further on.

Time-signals, as I have said, pass out of the South Eastern Railway by No. 3 wire, to the great Central Telegraph Office, once

in Telegraph Street, now at St. Martin's-le-Grand. Whence they require to distribute a large number of signals at the same time. This is a drawing of their instrument which is very complicated, as you see: but the broad principle is to receive a signal from Greenwich, and to distribute it by arrangement in all directions on their wires. A gun is fired by Greenwich signal at Newcastle, and, in fact, many other places. Here is one of Professor Abel's fuses with which the guns are fired, which is put into the touch-hole of the gun, and the telegraph wire passes through it, and when the proper current of electricity is sent the fuse explodes. If the gun in these grounds fires in a few minutes it will be fired by a fuse of that kind. The Astronomer Royal has been very anxious for many years past, but has not succeeded in inducing the Government to see it with him, to drop a ball daily at Start Point, the extreme south-west of England, for the use of ships; so that having taken their chronometer time at Deal, in passing out they might correct their chronometer time at Start Point. That still remains to be done.

Now I must say a few words as to electric clocks. Electricity is used with clocks either for driving them, that is taking the place of a spring or a weight as the motive power,—or for correcting them and putting them right when they are wrong—or for controlling them. There is, as you have heard, a large set of electric clocks in the Royal Observatory, moved by electricity distributed from one chief clock, with which each clock is kept always true. I have now, and have had since the year 1851, a set of four such clocks in the Telegraph works at Tunbridge, and they are constantly going. One serves the telegraph office, one serves the station, another serves our workshops on the inside and another one on the outside. I have made a calculation of the amount of electricity, or of the amount of zinc—which is the most convenient form for estimating it—consumed in working that set of four clocks, with which you, Mr. Glaisher, have been very familiar from the first. We require 30 cells of the platinized graphite battery, and consume 13 ¾lbs. of zinc per annum. Clocks however, are rather troublesome and expensive to move by electricity, because of the large portion of each minute that the electricity is on duty. I have here a memorandum showing

that for more than two-thirds of every second, or two-thirds of a year, the battery is in circuit and in action.

Then, again, clocks are corrected by electric currents. An ordinary clock is made to keep time as near as may be, and is corrected by electricity. The one referred to above in Lombard Street has been working since 1857 in this way. On the arbor of the minute wheel is a sort of V, and of course if the clock is a little too fast or too slow that V will be a little to the right or a little to the left of the vertical at a certain time. The keeper of an electro-magnet carries a solid V. When the signal comes from Greenwich at noon down goes that solid V into the hollow V, and thrusts the V fork backwards or forwards, and puts the clock right once a day.

The electric clock at Ashford was put right once a day in rather a peculiar way. We made it gain a little,—rather more than a second per day, and we so arranged it that, when it showed 1 P.M. o'clock, a pin pushed a spring aside, cut off the electricity, and the clock stopped. The 1 P.M. signal from Greenwich on its way to Deal, in passing, turned the electricity on again, and at 1 o'clock the clock started fair again, showing true time.

In clocks controlled by electricity, you start with a good regulator, having a seconds pendulum, and you get it to show very fair time. Then there are two or three ways of arranging the application of electricity. The pendulum carries a magnet, presenting its poles to a core-less electro-magnet. Electricity is sent in alternate seconds through the wire of this magnet from a good clock, and if the pendulum is disposed to go a little too fast or a little too slow, losing or gaining a few seconds, the currents of electricity check or control it, and thus you have a clock showing true time. The clock at London Bridge Station in connection with the Royal Observatory, has been used for many years, and frequently goes for several months, notwithstanding all the little hitches that may occur on a railway, with the breaking of wires and other matters, and shows precisely the same second with the Royal Observatory; and I believe it is doing so at this moment. The pallett-arbor of this clock carries a pair of bar-magnets, which oscillate above the poles of an electro-magnet, that receives controlling currents from

Greenwich. The wire of this clock does not stop at London Bridge; it continues along to the East India Stores, in Belvedere Road, Lambeth, and passes through a clock which it controls there for the use of the Astronomer. It was established, with our help, by the late Colonel Strange, F.R.S., and the clock is kept correct with Greenwich. Then the wire comes back and goes through the streets, to Mr. De La Rue's factory in Bunhill Row, where there is also a clock, which he exhibits to the public. That also makes accurate beats with the pendulum at Greenwich. There is an ingenious little plan for letting us all know, as we cannot tell without it, unless we have another wire and instrument, whether the clock is behaving properly—that is, one of the seconds does not come, or rather as we do not take them every second, but every even second, one of them (that is the one two seconds past the hour) does not come: it is cut off at Greenwich; so that by noting that the first signal that follows the blank shows the clock four seconds past the hour, we know that the clock is perfectly right. I cannot attempt to go into the details of a thing so intricate as the trains of wheels which are used for this purpose. The Chairman reminds me, for you see he is as well acquainted with these arrangements as I am, of the ball at Deal on reaching the bottom of the mast, touching some springs and sending a signal back, before we have broken the wires again, to the Royal Observatory, as a proof that it has done its work correctly and has dropped the ball. The Astronomer Royal is also able to see from the time at which the signal arrives whether it is working properly. It generally gets back in fifteen or sixteen seconds: if he were to see it in five or ten seconds he would be aware that the apparatus was not well adjusted, that the ball fell too fast and might do damage to the building. On the other hand, if it were thirty or forty seconds before the signal returned, he would know that the piston required greasing or something of the sort. With regard to the Westminster clock. The original proposition was that the Greenwich signals should be sent to Westminster, and to the Royal Exchange. It was not carried out to the Royal Exchange, but the Westminster clock receives signals systematically, and also sends back a record to

the Royal Observatory, which record is kept; and Sir George Airy, in that building on the top of Greenwich Hill, knows all about the clock at Westminster, much better probably than those who are immediately under it. I regret that you have not been alarmed by the gun firing at 9 o'clock, for it is now five minutes past nine; but there were some doubts about it, because the officers of the Post-office who were acquainted with the wires happened to be away, and the gentleman who is now present was not quite sure which was the proper wire without a little trial.

I will just say one word for the information of those who are unaware of it, how they find out the time at Greenwich; because it is obvious to you that they must find the time before they can send it. On the diagram before you, the circle with several lines across it may be taken as a transit instrument directed to the south, where an observation is taken of the sun or of any one of a large number of stars, whose time of passing the meridian is known. Day or night, it is immaterial; the star is watched in passing those wires, and the time of its passing is taken; in fact it is printed upon a drum in another part of the building, by touching a spring with the finger, by the person who is watching the telescope; then certain calculations are made for reducing this, which is sidereal time, to mean time, and the Normal clock which is used for distributing the time is set right to mean time. It is always set right before 10 o'clock, and again at the important times of the day. Ten o'clock is of importance for all the post offices in the kingdom, and 1 o'clock for dropping the ball at Deal. I have a drawing here of every detail of the transit instrument, used for observing the passage of the sun or stars.

Time has been measured in various other ways, which it would take a long course of lectures to go into. Sir Charles Wheatstone very early measured the time it took for the electric spark to travel: he got out the result, which we well know, as 225,000 miles in a second. In front of the 81-ton gun at the Royal Arsenal they have a couple of screens of wire at a distance which is known; the shot is fired through these screens, and

when it breaks the first it cuts off electricity, and then breaks the second and cuts off electricity there. Then there is an ingenious instrument at some little distance off in an office, which records most accurately the very great velocity of the shot, through that short distance of a dozen or so of yards. The first wire that is broken causes the drop of a rod of iron, which had been held up by an electro-magnet and drops when the current breaks; another rod is dropped when the second screen is reached, and it is so arranged as to make a mark on rod No. 1. They then refer to a scale, and are thereby enabled to measure with the greatest accuracy these high velocities. I would refer those, who would know more than could be expected in this one lecture, to Hughes' Reading Books, vol. iii. pp. 322–7, 1856; to De la Rives' Electricity, vol. iii. pp. 463–84, 1858. Mr. Ellis, of the Royal Observatory Electrical and Time Department, delivered a lecture before the Horological Institute, on the 24th February, 1865, which is reported in the April, May, June, and July numbers of the *Horological Journal*, and copiously illustrated, and contains in detail most of what I have given you very briefly. In *Nature*, on the 1st April, 1875, and on the 18th May and 1st June, 1876, there are also some exceedingly good accounts of matters connected with the Observatory at Greenwich, and they also are well illustrated. Professor Grant, of the Observatory at Glasgow, printed a letter only last week, addressed to the Provost of Glasgow, on the 17th May last, the purport of which is to show that it was really time that the authorities of Glasgow should provide for the time-signals which the Observatory there are supplying, as the Royal Observatory is here, at their own cost, to thirteen clocks in Glasgow, controlling by electricity included. The description of the chrono-trepeter, which I passed round the room, is a pamphlet by Mr. Varley, but I cannot give you the date of its publication. It was printed by Waterlow. Then there is a good description of the Greenwich transit instrument, which is probably in the libraries of all the scientific societies; and there are the annual reports of the Astronomer Royal, which are in the volume I have been quoting from.

Before I take my leave I have to thank you for listening so

patiently to a lecture which has so few popular attractions, and which has been, as I warned you it would be, very much a matter of dry detail.

The CHAIRMAN: Ladies and Gentlemen,—I scarcely need rise to ask you to thank the lecturer, for you have already done so, but I may, perhaps, be permitted to make a few remarks. As a consequence of this great system, for great it is, of sending time-signals, not only over all England but almost all over the world, we have been enabled to determine the longitude of places with a greater degree of accuracy than had hitherto been attempted. Even at the present moment the longitude of Vienna is being determined; that is to say, if the sky is clear at Vienna and a star is observed to-night on the meridian, a signal is sent to Greenwich, and is recorded there; and if the star should be observed at Greenwich to-night, the interval of time between the two is noted, thus showing the exact difference of longitude between the two places. Besides, the time-signals have a great educational influence upon the country. I have been, when a young man, in many villages where the people did not know the time to within two hours; but now, I ask you, is it possible to go to any village, or any place in the country where the time is not known to within as many minutes? Then, consider how punctual it compels us all to be, for the railways start their trains by Greenwich time; and therefore we are all compelled to be punctual, which is very different from what people were when I was a boy. That alone is a great blessing. It is impossible to trace all the good effects, for they appear in so many different directions. Things were otherwise at the date of the conversation to which I have referred, and which took place at Tunbridge, where I happened to be upon some other experiments with Mr. Walker. I may mention one other matter which has come under my notice as Chairman of the Meteor Committee of the British Association. You know that if a meteor be seen the observation is of no value (as we cannot calculate the distance of the meteor from the earth) unless we know the time nearly accurately; and it is quite common now, if anybody sees a meteor, for him to look at his watch and go as soon as he can to the railway station, and by that means he is

enabled to send us the time at which the meteor was seen, and it is by means of such observations that the tracks of many meteors have been observed, and that the progress of meteoric astronomy has been rendered so rapid in recent years. Were I to mention a number of other instances of the kind that occur to me I should indeed tire you.

I am desired to tell you that the clock-signal was received, but that the clock failed to act; but as we know that it drops a ball every day at Deal, and that there is a gun fired at Newcastle every day in this manner, you can easily believe that it is only a little inexperience which has caused it to fail here to-night. However, you have had the benefit, in consequence, of a little longer lecture, and I will now only ask you again to express your thanks to Mr. Walker for giving us the result of his experience, for that is what he has done to-night; and any one who will tell us faithfully and truly his own experience in connection with the history of any matter of this kind, does what is of infinite value, and prevents a great deal of labour that would otherwise have to be encountered in years to come by those who may wish to learn the real history of the past.

REFLECTING TELESCOPES.

By The Earl of Rosse.

July 18th, 1876.

Sir Henry Cole, K.C.B., in the Chair.

The Earl of Rosse: Mr. Chairman, Ladies and Gentlemen,—The subject on which I propose to address you is one of very great extent, and the immense variety of the applications of the telescope, of course, very much complicates it. Its uses are so various that it will be difficult to explain all the details of the construction of the telescope without going into practical astronomy to an extent which would be far beyond the limits of the time which we have at our disposal this evening. My task is made somewhat easier because the able astronomer of Oxford last week gave a lecture on the ordinary telescope of observatories. I was not here, but I believe that he dealt with the meridian instruments and the instruments that are most used in public observatories. Therefore, I shall apply myself to the special branch of the subject—that of reflecting telescopes, and only allude to refracting telescopes where it is necessary for a comparison of the two.

My subject I propose to divide into two. The nature of the reflecting telescope and its applications, its special uses for particular practice of astronomy compared with the refracting, I will first speak of; and then I propose, as far as time will admit, to go into the leading points of the construction of specula and the best methods of mounting telescopes.

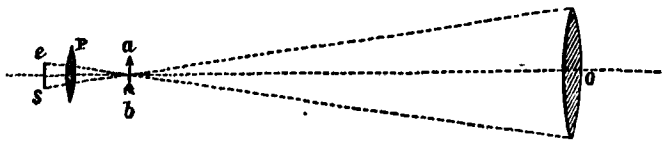
We shall all be agreed that the telescope may be defined to be an instrument for the better seeing of distant objects; but beyond

this there is a great deal of misconception as to the function of the telescope—as to the way, in fact, in which it enables us to see better distant objects of various characters. I find that a general notion of the telescope is that it is a magnifier of distant objects, and I have it constantly brought to my mind that that is the very general idea among those who have not specially studied optics. From having had handed down to me the two largest telescopes in the British Isles, one of which after thirty years is still the largest in the world and the second larger than any now in existence in the British Isles, I am constantly asked the question, "How much does your telescope magnify?" And this is about the only question put by people who wish to get some idea as to the power of my telescope. Now, magnifying is not at all the only thing that the telescope does, and it is not a property peculiar to large instruments. Small spy-glasses may magnify as much in one sense as the largest telescope. But there is another function which the telescope has to perform—that of gathering in a larger amount of light into the eye than the eye can take in when unaided. If we look at any object with the naked eye, if we look at that clock, for instance, all the light that comes to us from the face of the clock is simply what falls on and enters the pupil of the eye. The pupil of the eye is estimated to be about from one-fifth to one-tenth of an inch in diameter, according to the light which falls upon it. The stronger the light the smaller the pupil of the eye becomes; it is the means by which the eye adapts itself to varying circumstances and enables it to stand a strong light, and to see an object in a strong light, without injuring the eye. And, at the same time, there is the power of expanding the pupil to gather in light from a faint object when the light is feeble.

Now, the magnifying power of an instrument is very easily ascertained. It is obtained by dividing the focal length of the object-glass, by the focal length of the eye-piece; or, if we do not use an eye-piece—and it is not necessary to have an eye-piece to a telescope—then the magnifying power is the focal length of the object-glass divided by the distance at which we can see the thing distinctly with the naked eye,

which, with the average of people, is about ten inches distance. Thus, if we have an object-glass of ten feet focal length, the magnifying power will be twelve no matter what be the size of the object glass.

Now, the use of the lens is to enable the eye to get nearer to the image formed by the object-glass.



Here is the object-glass *o* of a telescope. It will answer just as well for my purpose as a mirror or a speculum, and here at *a b* the image is formed. Here (*a b*), the rays, coming from a distant object, meet in a point, and you bring this lens *P* nearer till you get it at such a distance that it pushes the image to ten inches distance from the eye. We see that as long as we keep the proportion of the focal length of the object-glass to the focal length of the eye-piece the same, the magnifying power is the same whether the instrument is a small opera-glass or the largest telescope that exists in the world.

Thus we can magnify, apparently, indefinitely with any lens.

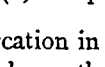
But what happens with magnifying powers? The more we magnify an image the fainter it becomes. We spread over a larger image the light which is gained and it becomes fainter. Therefore, unless the object is very bright—say like the sun—when we magnify we must, in proportion, enlarge the mouth of our telescope; so that it appears that magnifying power is unlimited provided that we have the aperture of the telescope unlimited. If we find a six-foot aperture insufficient, we may say that we will make one, if we can, twenty feet in diameter, and see farther into space.

Now, this property of telescopes is an exceedingly important one, and it is one on which Sir William Herschel laid great stress, and which was exceedingly important for his department of astronomy and for that in which my late father himself worked.

Sir William Herschel called that property the "space-penetrating power." The larger the mouth of the telescope, the farther you can see an object of the same brightness off in space. Ordinarily we only take in the rays which fall on the pupil of the eye. When we present a mirror to it, we take in this whole cone of rays. If we want to see from twice the distance, we must have double as large a mirror to take it all in, and that is why Herschel spoke of the space-penetrating power. The larger your object-glass, in general, the farther you can penetrate into space.

Now, I said that there is no limit, in one sense, practically, to magnifying power. The shorter the focal length of eye-piece, the higher the magnifying power. But there is a limit to the lowness of the magnifying power. The rays from every point of the image diverge and make an angle equal to the angle subtended by the object-glass at the image. The farther we follow these lines the larger that cone of rays is, and it will ultimately become too large for the eye at *e s*, if it is drawn too far back. If we use no eye-glass, the cone at the least distance of distinct vision will be too large to enter the pupil of the eye. If the magnifying power is less than, say, fifteen or so, with an aperture of three inches, then the eye will not take it all in; but by increasing the magnifying power we arrive at a point, which I will call the least magnifying power, where the eye can take in the whole of the light of the object-glass. That least magnifying power is equal to the aperture of the object-glass or speculum, divided by the aperture of the pupil of the eye. Therefore it appears that we must be able to magnify to that extent in order to utilize the whole mirror. If we have not that magnifying power, though we have a large glass or speculum, we really only observe with that light which comes from the more central parts of the object-glass or mirror.

There is a practical limit to the size to which your telescope is useful. I dare say that many of you may have noticed that the air trembles over a hot beach on a sunny day. That trembling is caused by hot air and cold air mixing together and causing the air to describe a crooked path. Well, the more you magnify the more you magnify that trembling, and you arrive at a limit very soon where the shaking is so great that though you have the

object larger, it is so confused and indistinct that you gain nothing by magnifying it. We shall see, presently, the only way in which that difficulty may be got over. Herschel has remarked in respect to the space-penetrating power, as he calls it, that the power to see a large faint object when you increase the aperture enabled him to see a church clock when he could not see the steeple with the naked eye, although the steeple was quite large enough, and although even the clock-face was large enough if the light had been bright enough. As an example of the special power on some objects which that large aperture gives, here is a drawing of the nebula in Orion. I have not any drawing here to compare with it. Herschel, Lassell, Struve, Lamont, and a great many observers, have made drawings of it; but this is a drawing which was done with the aid of the six-feet reflector some years ago, and it contains by far more detail than any other drawing. There are also many objects in the heavens which we call spirals. They are something of this shape [*a*], and this shape [*b*]. The spiral configuration was first perceived by my father, with his six-foot instrument. Other observers had looked at these objects. Herschel had found them and catalogued them, but he had not discovered them to be spirals. The largest spiral was something very small as seen by the reflector. Herschel drew (*a*) simply of this shape [], just a small ring with a bifurcation in it.

Now, having shown the advantage of the aperture, I will come to the next part of my subject, namely, the facility with which a large aperture may be obtained with the reflector. This is the great reason for adopting the reflector in these days. Newton, in his day, constructed this small telescope which belongs to the Royal Society. It was the first reflector, I believe. But he contructed it to get over another difficulty—a difficulty which occurs in refractors, but not in reflectors. In both you have some difficulty in getting the rays of one colour to come to a point. But in addition

while in the case of the reflector, if you get, say, the green rays to come to a point, you get all the rays to come to the same point; in the case of the refractor, the violet rays are more refracted or bent than the others, and the rays of various colours do not all come to one and the same point. It was one of the great problems how to get all the rays of different colours to come to a point; and it was Dollond who discovered an approximate solution. He succeeded in solving the difficulty very nearly, and others improved upon the construction of his object-glass. His object-glass is formed of two kinds of glass—crown glass and flint glass. I shall not have time to go into the construction now, but by this means the chromatic aberration, or the straying away of a ray of one colour from a ray of another colour, is corrected. There is still a point in which it is imperfect. You can get, say, the green rays to go to one point and the red rays to go to the same point; but it does not follow that you can get the violet rays to go to the same point, and the rays which act on the photographic plates do not, necessarily, come properly to a point when the rays that affect the eye do. But more of that in a few minutes.

We have seen that the amount of light gathered into a telescope and what comes into the eye is roughly proportional to the surface of the glass, or speculum that catches the rays that fall upon it; but here is a lens upon which forms an image of the gas over there. Of whatever light falls upon it the greater part comes through; but about 5 per cent. is thrown back from the first surface and is lost. In the same way, of what comes through there is 5 per cent. lost on the second surface, and that is thrown back and lost; so that 10 per cent. of light is lost in passing through a lens of glass. In the case of a speculum there is a larger amount lost. There is only something like two-thirds, or about 63 per cent. which is reflected. And then when you use the second reflector there is only 63 per cent. of the remainder which is reflected; so that there is a larger amount lost in a reflecting telescope than a refracting telescope when the telescope is small. But the percentage of loss—roughly 35 per cent.—* by a mirror, remains the same whether the mirror is large or small.

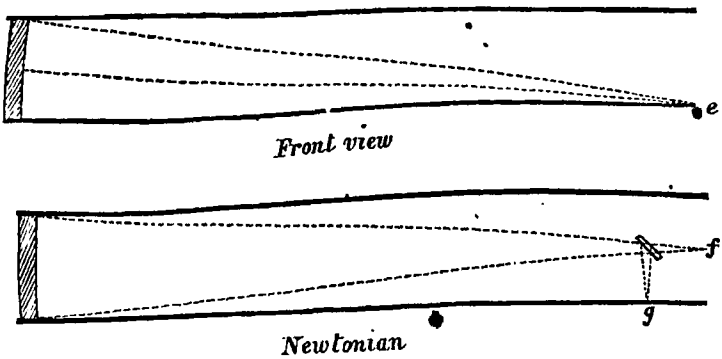
* 20 per cent. only is lost in passing through an uncemented achromatic object-glass.

There is another cause of loss in a refractor, namely, that if the glass is not perfectly colourless there is a loss of light in addition to what is reflected. In passing through the glass some of the light is absorbed, and the larger our lens the greater the thickness of the glass required and the greater the amount of absorption; so that if a refractor be superior to the reflector in proportion to its size when it is small, it does not at all follow that when the refractor is very large that superiority will still continue. It has been estimated that a 48 inch speculum is about equal, all things considered, to a $35\frac{1}{2}$ inch refractor; so that a smaller refractor, it seems probable, will do the same work; but that does not seem to be at all established, because it has not been thoroughly tested.

There is a further difficulty, namely, that you cannot support the glass to prevent it from bending, which, if I have time, I will go into presently. The mirror you can support. I may mention that one very great point in favour of the reflector is that even if you succeed in making a refractor of from 35 to 36 inches diameter of object-glass—which is exceedingly doubtful, none ever having been made of that size—the cost would be something very great. It has been estimated that a 35 inch refractor would cost £25,000. I believe that, taking all things into consideration, that is considerably within the mark, even if it was accomplished. The cost of a 48 inch reflector is about £5000 or £6000. There is considerable difficulty in making the mounting in addition to making the object-glass; although the object-glass is very much lighter, the mounting requires to be strong, and the building larger owing to the greater focal length as compared with a reflector. I have looked into the proportions adopted by a number of first-rate makers, and the focal length varies from 16 to 18 times the aperture. Herschel made his reflectors with focal lengths in the proportion of twelve times the apertures. Lassell has made the focal lengths equal to ten apertures, and in those at Parsonstown the focal length is equal to nine apertures.

Now, there are two ways in which a reflector may be used. There is that which was commonly adopted by Herschel, namely,

what he calls the front view. Here is the speculum. Here (*f*) the rays come to a focus. Newton placed a plane mirror here and threw



the light (to *g*) to the eye sidewise, as you will see in this telescope. On this plan you lose a second, 30 per cent. of the light coming from the large speculum; but by using no plane reflector, tilting the speculum, and putting the eye here (*e*), at the side of the mouth of the tube, you save all that; but you lose at the same time through aberration or "astigmatism," by the head intercepting the light, and from the breath causing disturbance of the vision. Both constructions have their advantage. The "front view" gives more light with faint objects which are ill defined, like the tail of a comet, where you do not want the sharpness of vision which you require in the case of a star, or, most of all, in the case of a double star, where you want to separate the two components. But in the case of the nebulæ you do not require that so much; therefore, the disturbance of vision in the mirror, and throwing the optical arrangement crooked, so to speak, is very much less important than the loss of light in some cases. Herschel, in consequence of the long focal length he employed, was able to use the "front view," a great deal more than we have at Parsonstown. We find that, for most purposes, the second reflection is the best.

I have spoken in reference to nebulæ. There are other special subjects for which the reflector is peculiarly adapted, about which

I wish to speak a few words. Lately to the work of the observatory there has been added observation with the spectroscope. It is a new branch of astronomy, and I have had very little experience in it myself. I dare say that many here present know that the spectroscope requires the light which is to be examined to pass through a very narrow slit, in a thin plate of metal. If you examine a star you must get the light of that star to pass through that slit; and it is evident that the motion in following the heavenly bodies must be so arranged that you get the light constantly passing through that slit, which is a matter of extreme difficulty; and I do not believe that any clockwork arrangement, without help, has ever kept the instrument in such a position for any length of time so that the light will pass continually through the slit. A great deal may be done with a moderate sized instrument by just pressing the finger upon it, for the metal yields a little. But in the larger telescopes this is attended with greater difficulty. For other reasons, which it would take too long to go into here, spectroscopy was not a suitable subject of investigation for my large reflectors; and I must say that for the spectroscope, at present, the refractor seems to have the advantage in consequence of there being less absorption of the rays in passing through a small object-glass than in reflection from a speculum. You get a brighter spectrum, and I think, on the whole, the large-sized reflector does not give it advantages which, if any, are at all equal to those that are given by it in other investigations.

I now come to another line of investigation which, I think, is peculiarly suited to the reflector as opposed to the refractor. That is the examination of the heat of the heavenly bodies; and that being a subject to which I have devoted special attention, I will ask your indulgence for a few minutes while I allude to it. I have said that 10 per cent. of light is lost in passing through a single lens of glass; but that is not at all the case with heat. A very much greater proportion of heat is stopped by the glass. While 10 per cent. of light is stopped in passing through an ordinary glass, 20 per cent. of the sun's heat is stopped; and when you come to bodies of lower temperature, like the moon, from 10 to 20 per cent. only gets through; that is to say, 80 or 90 per cent.

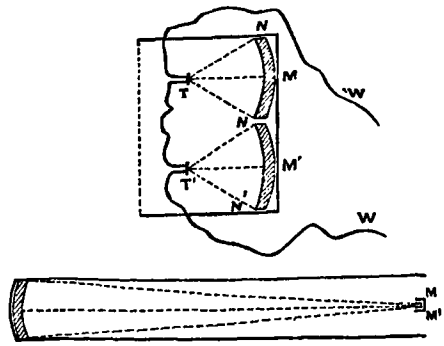
is stopped by the lens. From bodies which are not luminous at all, such as a vessel containing boiling water, or a heated mass of iron below red heat, a mere trifle gets through, or it is doubtful whether any gets directly through the glass.

Now, this fact made the reflector peculiarly suitable for working at the heat of the moon. No one, I think, had tried any apparatus of large size before myself except Melloni, whose name is well known in connection with the thermo-pile, an instrument for measuring very minute quantities of radiant heat. He took a compound lighthouse-lens of about 3 feet diameter—and he placed the thermo-pile in its focus. If he got any deflection it was exceedingly minute; but he was trying to measure 10 or 20. per cent. only of the whole instead of, say, 60 per cent., had he used the reflector. My reflector which I used for that purpose was not my large one. It was my three-foot reflector, and, consequently, my concentrating power, if it were not for that difference between the reflector and the refractor, would not have been superior to this lens except in the one point. Now, this is the actual apparatus which I employed on the moon. The three-foot mirror would be nearly as far [27 feet] as the corner of the room. I do not know what distance that is. This was presented to the speculum. The light of the moon fell on one of these mirrors which you see here, and was concentrated on the face of that thermo-pile. The moon's image was just about the size of one of these reflectors. These reflectors are very concave, and they threw the light on the thermo-piles fixed in these little brass tubes, so that by that means I threw on the faces of the piles, the light falling from the moon on a surface of three-feet diameter, on to spaces of about the third of an inch diameter, and allowing for the loss of light on the mirror, I concentrated it about three thousand times. This gave me a great advantage. I was able to measure the moon's heat with a very fair amount of certainty, and also to measure it when it passed through a sheet of glass, a sheet of glass being passed across the face here, and I was able to estimate the proportion at various ages of the moon. This diagram represents the apparatus on an enlarged scale.

Here is the thermo-pile [T, T']. The red line [N T, N' T'] re-

presents the reflected ray falling on the face of the thermo-pile. Here are the wires [w w'] which went to the galvanometer which

Thermo-piles and mirrors.



Telescope with thermo-piles fixed in focus of speculum.

indicated the electric current arising from the source of heat. I used two piles to avoid disturbing causes owing to the currents of hot and cold air which take place. This diagram represents the the results.

Here is the first quarter of the moon; here is the full moon; and here is the last quarter. The height here up to this line represents the total amount of heat at various ages of the moon; and the lower line represents what passed through glass, and this is all that Melloni could expect to get from his apparatus.

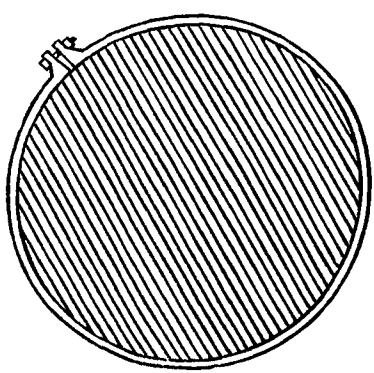
I will now pass on, as the time is advancing, to the second part of my subject. Great difficulties were experieneed at the first in making mirrors of large size. This largest mirror on the table is one of the first attempts to construct a mirror of any size.

It may, perhaps, be considered behind the day and in the nature of a curiosity; but I propose, as far as possible, to direct your attention to the objects in the exhibition, and, therefore, I will just briefly describe it. [Compound two-foot mirror with brass backings was described.]

One of the greatest difficulties in constructing a mirror of speculum metal is the extreme brittleness of the metal. The least

knock will break it, and it is very difficult, when it is of any size, to prevent its cracking in cooling. It is far more brittle than a piece of glass, when made of the proper composition, and it requires to be annealed with great precautions in an oven. The mirror of the six-foot reflector took five or six weeks to cool, so great was the care necessary to avoid cracking. The proportion employed and found best by my father was about two parts of copper to one of tin. The exact proportion is $2\frac{1}{8}$ or $2\frac{1}{7}$ of copper to 1 of tin. That is an exceedingly brittle metal. Herschel, when he endeavoured to go up to the larger sizes, altered the quality of his metal, to enable him to cast a perfect mirror, which, though inferior in proportion, would still be more powerful than the smaller one. Accordingly, as he came to a larger size, he increased the proportion of copper from two to one, and gradually got it more and more, till his 48-inch speculum was four to one, and it in consequence tarnished very rapidly and was always, more or less, of a bronze colour. I may mention that it more nearly approached bronze than the great bell of Westminster. I believe that the proportion in that bell is three to one. It is a very brittle metal. I have here a piece of speculum metal which, I have no doubt, if it were held over a candle for a few minutes, would all fly to pieces, and when struck with a hammer will break to pieces if it is of the proper quality.

One of the difficulties in casting a speculum was to avoid 'cool-



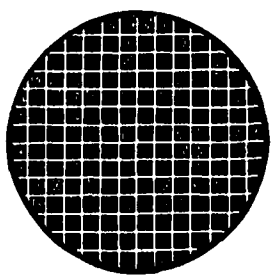
Bed of hoops, seen from above.

ing it unequally; at the edge, probably, more rapidly than at the centre. The equal cooling was effected in earlier times by pouring it on a cast-iron bed with a mould-sand ring round it. This had the objection that the air imprisoned in the metal could not be driven out as it could through the sand, and an expedient was hit upon, which was my father's invention, and was what we may call the bed of hoops.

Here is the bed of sand, here is the bed of hoops, a little larger than the speculum, and the bottom, instead of being made of cast iron, was formed of a number of pieces of iron laid edgewise and close together, and wedged up by another piece. The metal falling on to this, was instantly cooled, so that it could not get through before it was cooled; but the air had time to get through, and in that way we obtained a better result than any which had been got in other ways. Mr. Lassell adopted the method of pouring the metal in at one side of a tilted mould which was brought into the horizontal position after the metal was in, so as to make it flow over to one uniform depth. That is attended with some difficulty in a very large casting, but he produced very good specula in that way. The bed of hoops is, I believe, still considered to be the best mode and was adopted by Mr. Grubb in casting specula for the great Melbourne Reflector. We will just try this metal. [The lecturer struck a piece of speculum metal with a light hammer and thereby fractured it.] You see, it is a much smaller blow than would break a piece of glass. You remark that the colour looks very white; though it is two to one of copper. You will notice from the surface that this has been cast on a bed of hoops. There are some lines or inequalities on its surface which are caused by the bed of hoops. In the case of small mirrors of this size, we just put them into a mass of warm peat ashes and let them cool there for three days.

I must pass rapidly on. In lenses and object-glasses the only surface attempted is the spherical surface. It is much easier to obtain than any other, and it is the only one that can be obtained by grinding. Those who look into the subject will see at once that spherical surfaces are the only ones that can be got with any

accuracy and certainty by grinding two surfaces together. In grinding the spherical surfaces, however, the same curve is always obtained, and in order to obtain any other curve you must do it in polishing. The proper curve for a reflector is the paraboloid. It is the curve whose section through the axis is a parabola. Of its properties I will simply say that it differs from the sphere by having a less curvature towards the edges. Therefore, in polishing, the aim is to grind down the edges more than the centre, and that is effected by taking advantage of the elasticity of a pitched surface on a tool similar to that used to grind with. Here is a representation of a pitched surface. The black squares are bits

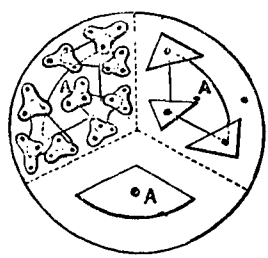


of pitch, and it is necessary, in order to obtain a true surface in polishing, as also in grinding, and to prevent the surface being convex, to have grooves cut to allow the excess of water and emery to pass along the grooves in the grinder to another part. And in polishing you allow the same groove for the polishing powder, and also allow the excess of pitch to squeeze out, so that the whole thing may take the shape of the speculum. The whole thing is produced by the polisher passing out beyond the edge of the mirror and expanding gradually, and it wears the edge down by its elasticity more than it does the centre.

There are so many points to be considered, that I can only just go rapidly through them. There are many defects in polishing which are very difficult to get rid of. You may, by having the pitch too soft, wear the soft parts of the metal out more than the hard parts. If you have the pitch too hard the polisher will not adapt its shape to the speculum properly. If you have the air

too damp the polishing powder will dry too slowly; and if you have it too dry the powder will dry too quickly.

I have now to speak of the method by which mirrors can be supported at the back when of large size. A mirror of this kind, which is what they call a composite mirror, as I should have stated earlier in the lecture, is formed by putting small blocks of speculum metal upon an alloy of zinc and tin of the same co-efficient of expansion. It is very rigid. As an engineer would make a stiff iron casting, it has ribs underneath, and the thing is very much stiffer than you could get it by a solid block of metal like this piece. This is the support.



It is made to stand steadily, on a system of triangular levers resting steadily on three primary points [A, A, A]. By that means it will be evident to any one who considers it from a mechanical point of view, you can support that little triangle and this independently, with an equal pressure, and you get the whole thing quite as free from strain and much more steady in its position than Herschel succeeded in doing with his smaller specula on a cushion. His was the simpler plan, but it is found to be insufficient.

There is another method of constructing specula which I will explain—namely, of silvered glass. I think M. Foucault, a Frenchman, first introduced it. Steinheil, and several others, worked at it. This is one made by Mr. Browning, of London. It is a disc of glass which, as the light has not to pass through it, does not require to be as pure as for an object-glass, and it is ground to the proper curve, as we grind our speculum, and it

is turned upside down on a bath containing a salt of silver, and the silver is deposited on the under side; so that you not only get a looking-glass, but you can polish the back with a delicate rubber and a little rouge and get the back surface as bright as the front. It has one great advantage over the metal reflector, and that is, that, although there is a difference of opinion as to what the actual amount of light is which it reflects, that light is admitted to be rather more than from the metal reflector. I believe myself that though it is not as much as solid silver, it reflects, perhaps, 85 per cent. as compared with 62 or 63 per cent. of speculum metal. Pure silver reflects 90 per cent., so that if we could construct reflectors of pure silver, we should at once have a reflector superior in every way to the refractor.

This is a polishing machine said to have been used by Sir William Herschel. These are the wheels by which the speculum works on the polisher. The polisher goes round, the speculum moves in straight lines, and the speculum goes round too by means of a rachet. This rachet moves the speculum round slowly, and the polisher moves round more rapidly. In the case of our machines the speculum is undermost, and moves round in the same way as the polisher here. The polisher is allowed to go round by its own action. It works itself round, and the polisher goes backwards and forwards for grinding, or else moves in a circle; but the difference is that the stroke is the same at all times. In the case of our machines, instead of moving this way in the same line it keeps moving this way and then back again, towards and from the centre of the speculum, and by that means you get more variety of motion. You produce strokes at every distance from the centre of the speculum, and you get a far truer figure. There is another plan which has been adopted at Parsonstown, and which has been modified by Mr. Lassell, in which the strokes are circular, round a fixed point.

And there is a further improvement upon that which I believe has been employed by Mr. Lassell, and also in general at Parsonstown. Instead of this polisher going round the centre of the speculum, practically you vary this point from the centre—perhaps half way to the edge. In fact, in going from the plan that Herschel

found the only successful one—namely, that of polishing by his hand or by the hand of the workmen—for his own unaided hand was not powerful enough—to the use of this machine, you must try to get a variability of the actual strokes, and at the same time, an invariability of the average position and length of strokes.

Now to come to the mountings.

This is one of Sir William Herschel's. It is of the form which we call alt-azimuth. It moves in altitude by screwing this up and down, and I think also by this. But the alt-azimuth has one very great disadvantage. You cannot with one motion follow the motion of the heavens. The apparent motion of the heavens is round the axis of the earth, which in this latitude is $51\frac{1}{2}°$ in altitude. The Equatorial you set in Polar distance by means of one motion [this,] and to follow the object you have simply to turn it round this axis [polar axis].

In this, knowing from your catalogue the position of the heavenly bodies—what we call its north polar distance—we fix the telescope at that distance; and we turn it round to the proper distance east or west; and then by setting the clock going we can, by means of wheel work and a screw, follow the object, which it is very desirable, should be done with accuracy and uniformity.

This is the ordinary, the most common mounting of telescopes of every size. It was first proposed by Fraunhofer, a German. It is, on the whole, the best. It has one disadvantage. When you come to the south point you have to turn it round so as to get the tube to the other side of the Polar Axis in this way, before you can follow the object on towards the West. That is rather a troublesome operation, and it is a greater disadvantage from occurring at that particular position.

This is a model of the great work of Mr. Grubb, of Dublin, which he has in hand—the construction of a large refractor for Vienna. The refractor—the largest yet attempted—has, I think, 27 inches diameter of object-glass; and much as I think can be done still by reflectors, I am glad to see this experiment tried on a large scale, and that persons are willing to undertake the serious task of constructing large refractors.

This, as I said before, is a model of the great reflector at Parsonstown. It was at first, and in fact still is, the only attempt which has yet been made to mount anything so large as a six-foot mirror. Though you cannot follow the object farther than from this wall to this, it still possesses some very great advantages. It is suspended by chains, and it is steadier in the wind than any instrument which has ever yet been made for the open air. You stand in these galleries sixty-feet above the ground. You look through the eye-piece here, and we stand in this gallery or that, according to the position of the object. It can be raised up and down by means of this windlass.

This is a model of the great Melbourne telescope, constructed by Mr. Grubb, who has made such a high name for himself in the construction of large instruments. It has a four-foot aperture. It has been erected in Melbourne, and has been worked for the last seven or eight years.

As that is the newest thing in refractors, so this (the new mounting for my three-foot reflector), perhaps, is the newest thing in reflectors. The former mounting (an alt-azimuth) was one which had the advantages which I spoke of just now, and the same disadvantages as Herschel's—namely, that you require two motions to follow the heavenly bodies.

This equatorial is of the Newtonian arrangement, but can be easily adapted for the "front view," and the observer stands here. As it is only just completed, or, I may say, not completed, it would be premature of me to go farther than to say that I hope, I have every reason to think, that it will be worthy of a place with the other mountings of large instruments. It has been constructed by a very able engineer.

To conclude. I have said that there is, practically, one great difficulty in carrying the power of instruments to penetrate into space farther than a certain distance, and that is, as we call it, the unsteadiness of the air—heated currents and cold currents of air, which prevent our using the minimum magnifying power perhaps five nights out of six. Now, the question is, What can be done to advance farther in penetrating into space? I believe there is only one way—namely, that as the atmo-

sphere is the great difficulty we have to contend with, we must endeavour to get away from the atmosphere as much as possible. An attempt was made many years ago to test the advantage of ascending to high elevations on the Peak of Teneriffe. The late Mr. Robert Stephenson, the engineer, kindly lent his yacht for the occasion, and Professor Smyth, of Edinburgh, went out and made some experiments on the Peak of Teneriffe. I think that the experiments were insufficient to test the matter properly, and a fuller trial should be made. The Americans are already talking of putting up an instrument of large size somewhere in the hill country—in the Rocky Mountains, and a large sum is in the hands of trustees, I believe, for the purpose; so we may hope that we shall before very long see the instrument erected. In Ireland we have even more cloudy weather, and more rain, than here, so we are very unfavourably situated, and the large reflector does not receive fair play at all, from the weather. But while the American Government are going in for such a large refractor, and while in the United States private individuals are talking of trying to advance farther in that direction, let us hope that our own country will not be behindhand; and many as are the subjects which the Government of this country are most liberal in trying to advance, let us look forward to seeing before long on English soil, perhaps in the hill districts of India, in a suitable climate, and at a suitable elevation, a reflector of large size which will surpass in its results anything that has ever been accomplished before.

THE GREAT AND LITTLE BASSES ROCK LIGHTHOUSES.

By Mr. J. N. Douglass.

July 24th, 1876.

Admiral Sir R. Collinson in the Chair.

The Chairman:—I have the pleasure this evening to ask you to listen to a gentleman who I am sure is perfectly capable of telling you fully what he is going to talk to you about, for I suppose there are few persons at present who have so complete a knowledge of lighthouse establishments as Mr. Douglass. Therefore without trespassing further on your time, I will ask him to begin his Lecture.

Mr. J. N. Douglass said: The subject of lighthouses is one of particular interest in this great maritime country; for to every part of the globe where civilization and commerce have spread has the advance been marked and facilitated by coast lights.

The class of lighthouses which are the subject of our lecture are those erected upon tidal rocks at some distance from the coast, and thus exposed to the full fury of wind and waves. The first of these lighthouses erected in this country was that upon the far-famed Eddystone, by Winstanley in 1696. This was a timber structure, ill adapted to resist the heavy seas to which it was exposed, for, during a furious storm in November, 1703, it was completely swept away, together with its builder, who had gone off to effect some necessary repairs. The second lighthouse, designed and erected by John Rudyard, was a combination of wood and stone, and a considerable improvement as regards form and stability over its predecessor. This work was commenced in 1706, and was first lighted on the 28th July, 1708. It successfully resisted the heavy seas to which it was exposed for 47 years, when, about 2 o'clock on the morning of the 2nd December, 1755,

the lightkeeper on duty, going into the lantern to snuff the candles, found it full of smoke. The lighthouse was on fire, and in a few minutes the fabric was in a blaze. Fortunately the lightkeepers were rescued, but Rudyard's lighthouse was completely destroyed. The third and present lighthouse on the Eddystone is the noble work of that eminent father of civil engineers, John Smeaton, who gave the subject the most careful investigation. Through the kindness of Mrs. Croft Brookes we have before us the first model of the Eddystone, which is stated to have been made by Smeaton's own hands. Smeaton carefully examined the plans and models of the two former lighthouses : by this, he sought to ascertain their defects, with a view to their avoidance in the intended new structure. In the course of the enquiry he became convinced that a great defect in the late building had been its want of weight, through which it had rocked about in heavy storms and would probably have been washed away before long if it had not been burnt, and he came to the conclusion that if the lighthouse was to be contrived so as not to give way to the sea, it must be made so strong as that the sea would be compelled to give way to the building: He also had regard to durability as an important point in its re-erection. To quote his own words, " In contemplating the use and benefit of such a structure as this, my ideas of what its duration and continued existence ought to be were not confined within the boundary of one age or two, but extended themselves to look towards a possible perpetuity." Smeaton had thus arrived at the firm conviction that the new lighthouse must be of stone, and, with regard to form, the idea of the bowl of a large spreading oak tree presented itself to his mind as the natural model of a column presenting possibly the greatest stability. Another point which he long and carefully studied was the best mode of bonding and dovetailing the blocks of stone to the rock and to each other in such a way as that not only every individual piece, but the whole fabric, should be rendered proof against the external forces to which it must be subjected. Smeaton commenced his work in the spring of 1756, and on the 16th October, 1759, the light was first exhibited, and the column still stands, after nearly a century and a quarter, a lasting monument to its architect and builder.

The illuminating apparatus of Smeaton consisted, as did its predecessors, of tallow candles weighing two-fifths of a pound each; these were mounted on a chandelier, with no optical agent for directing their light to the sea surface, consequently only a small portion of the available light was utilized for the benefit of the mariner, the remainder being wasted upon the sky and the roof of the lantern. This is not surprising when it is remembered that at that date nearly all our coast lights consisted of coal fires, and the science of lighthouse illumination was just dawning. From experiments which I have made with tallow candles of the dimensions of those used at the Eddystone, I find that their illuminating power was about $2\frac{4}{5}$ standard sperm candles, or English units of light; this would give as the aggregate power of the beam from the 24 candles 67 standard candles or units. The consumption of tallow I find to have been about $3\frac{3}{8}$ lbs. per hour. We have before us a reflector lent by the Trinity House, which was one of the earliest used in lighthouses. It was invented by William Hutchinson, and was first used at Liverpool about 1763, and afterwards at Lowestoft and other lighthouses on the English coast. The surface of the reflector is approximately paraboloidal in form and is covered with small facets of silvered glass. The reflector is furnished with a rude oil lamp, having a flat wick about $1\frac{1}{8}$ inch wide. I have measured the maximum intensity of the beam of light emitted by this reflector and find it to be about 81 standard candles. Probably when the reflector was new the intensity might have reached 100 candles, or about 36 times the intensity of the flame.

In the year 1810 the Trinity House substituted 24 sperm oil lamps and paraboloidal reflectors of silvered copper for the candle light at the Eddystone. One of these reflectors and lamps lent by the Trinity House is before us. They were constructed from a formula proposed by Capt. Joseph Huddart, F.R.S., and an elder brother of the Trinity House; they are still used in some of our lighthouses, and are known as Huddart's reflectors. By this improvement the intensity of the light at the Eddystone was raised to about 1125 standard candles, or about $16\frac{1}{4}$ times that of the candle light. In 1845 the Trinity House further improved

this light, installing a second order dioptric apparatus of the system of Fresnel, thus increasing the intensity of the beam to about 3216 standard candles, and in 1872 an improved lamp of larger dimensions was introduced, thus increasing the light to about 7325 standard candles, or about 109 times the power of the original candle light. This large increase in power, due to the aid of scientific instruments, is obtained at about one third the former annual cost for candles.

Smeaton's success with the Eddystone led to the erection of several similar works on the coast by the Trinity House and Commissioners of Northern Lighthouses. Among the earliest of these may be mentioned the Bell Rock and the Skerryvore, designed and erected by the Stevensons, father and sons, who have done so much for lighthouse illumination, and among the most recent and difficult are the Bishop and Wolf, off the West coast of England, and Dhu-Heartach, off the North-west coast of Scotland.

The Great and Little Basses reefs, which are the immediate subject of our lecture, are situated off the South-east coast of Ceylon; they are respectively about 80 and 100 miles eastward of Point de Galle and about 7 miles from land. The Great Basses Rock is about 220 feet long, 75 feet wide, and about 6 feet above mean sea level. The rise of tide averages 2 to 3 feet. The Little Basses reef is only awash at low water. Both reefs are composed of a hard red sandstone, and the surface is very rugged. These reefs are exposed to both the North-east and South-west monsoons, consequently the days available for working on them during a year are but few. If they had been situated about 50 miles nearer to or farther from Galle the number of days on which a landing could have been effected would probably have been doubled. The only suitable season for working is during the North-east monsoon, which commences in November and terminates in April; and the best months of the monsoon are the first and two last. During part of December, the whole of January, and part of February, the wind blows strongly from North-east, especially about 2 P.M., when a short quick sea, on reaching the shallow water near the reef, breaks

heavily on it, rendering it dangerous to approach in a boat. During November, March, and April the wind is variable; light breezes prevail frequently off the shore in the morning, and on the shore in the evening, with a calm at mid-day. The current sets westward during the North-east monsoon and eastward during the South-west monsoon; the rate is very irregular, sometimes varying from half a knot to four knots per hour during the same day.

The usual direction of the current is parallel with the coast. Towards the close of the monsoon the current is weak, and occasionally flows feebly in opposite directions during the same day. It attains its highest velocity to the westward in December, January, and the early part of February. The coast for many miles on both sides of the Basses is almost continually exposed to a heavy surf from South or South-west. It is very thinly populated and is without secure shelter for shipping. It was therefore necessary to establish at Galle the depôt from which the operations at the Basses were carried out.

The rapid increase in the shipping trade passing this portion of the coast of Ceylon and the number of wrecks occurring, has for many years rendered necessary the lighting of these dangers to navigation. In 1855 the late Mr. Alexander Gordon, C.E., was instructed by the Board of Trade to prepare plans and estimates for a lighthouse for the Great Basses. The design shown on the diagram was approved. It consisted of a cylindrical tower of cast iron, supported within an enlarged base of granite. The illuminating apparatus consisted of 18 paraboloidal reflectors, 21 inches in diameter, arranged in six groups of three each, thus giving six beams of light to the circle for a revolving light, the intensity of each beam being about 3375 standard candles. The estimate for the work was £33,946.

Early in 1856 operations were commenced in Ceylon. In the meantime the lighthouse and its illuminating apparatus were prepared in England and despatched to Galle. After three years, only a few landings on the rock had been effected, and nothing had been accomplished beyond the erection of a beacon mast and the marking out of the site of the proposed lighthouse. It was

found, in fact, that the difficulties to be encountered had not been fully appreciated before the work was commenced, and consequently the arrangements for meeting them proved insufficient. About £40,000 had been expended, and it was estimated that £20,000 per annum for five years would be required to complete the lighthouse.

The authorities, unwilling to incur so large an expenditure upon what seemed from past experience a doubtful chance of success, suffered the work to lie in abeyance.

In the year 1863 a light vessel was moored off the Little Basses, from which a white revolving light has since been exhibited. Various schemes where subsequently submitted to the Board of Trade for the erection of a lighthouse on the Great Basses, and in 1867 the whole question as to practicability, probable cost, and reasonable chance of success, together with the various proposals for its construction, was referred to the Elder Brethren of the Trinity House, which resulted in the adoption of the design by their engineer, shown by the diagram and model. This design, was for a granite structure, in which the granite base of the Gordon lighthouse (the only portion of the original work which could be made available) was proposed to be utilized. The scheme further included a lantern and dioptric revolving light of the first order, also a light vessel to be moored off the rock, for exhibiting a red revolving light regularly every night from the commencement to the completion of the work, and to serve as a barrack for the executive engineer and working staff. The total estimated cost of the work was £64,661. Mr. William Douglass, C.E., who had erected the Hanois and Wolf Rock Lighthouses in this country, was selected by the Trinity House as executive engineer for the work. The lighthouse consists of a cylindrical base of granite, 32 feet in diameter, and 30 feet high; on this is a tower 23 feet in diameter at the base and 67 feet 5 inches high. The thickness of the wall at the base is 5 feet, and at the top 2 feet. The accommodation for the lightkeepers consists of six rooms, 13 feet in diameter, and one at the base for water and coals, 12 feet in diameter. The lighthouse contains about 2,768 tons of granite. The stones forming the walls and floors of the tower are dovetailed

together, both horizontally and vertically, on an improved system devised by Mr. Nicholas Douglass, and first adopted at the Hanois Rock Lighthouse, Guernsey, in 1859.* On the upper bed and on one end-joint of each stone is a raised dovetailed band, and on the under bed and at the other end-joint is a corresponding dovetailed indentation. The indentation is made just large enough for the projection to enter it freely with a coating of Portland cement. When the latter has hardened the blocks cannot be separated without breaking the solid stone. The whole tower is thus rendered literally one solid mass of granite. The step-ladders for ascending from floor to floor are of cast-iron. The entrance-door and storm-shutters to the windows are of gun-metal. The use of wood has been avoided in the internal fittings as far as practicable, so that the building is completely fire-proof. The lantern is cylindrical, 14 feet in diameter, of the Trinity House pattern. The one for the Little Basses Lighthouse is to be seen in the Loan Exhibition, and is lighted this evening. The cylindrical lantern has the following important advantages over previous forms of lanterns with facets of flat glass: 1st. It is more perfect optically, the light from the central apparatus always falling nearly normally upon the surface of the glazing, and thus avoiding surface reflections, which not only involve loss of light but are often mischievous. 2nd. Less resistance is offered to storms. 3rd. The curved glass has been found by experiment to be 58 per cent. stronger for resisting external pressure than flat glass; and 4th. The vertically curved framing has a maximum rigidity for the support of the glass, and offers a minimum obstruction to the light emitted by the illuminating apparatus. To that eminent man of science, Faraday, who was for many years the scientific adviser of the Trinity House, is due the perfection of the ventilation of these lanterns. The requirements are, a perfectly uniform and plentiful supply of air to the large central lamp, and the prevention of condensed vapour upon the internal surface of the glazing. To meet these requirements, the external air is admitted at the ventilating

* Diagrams of this and other appliances referred to by the Lecturer were hung on the walls of the room.

windows of the service room under the lantern; thence it ascends into the latter through an iron grating, which surrounds the lantern floor, and upwards over the surface of the glazing through an aperture surrounding the ceiling to a space between the latter and the roof, and thence through the shaft, from whence it is discharged by the revolving cowl, the ascending current being invigorated by the heat of the funnel of the large central lamp. Hit-or-miss ventilating valves are placed round the pedestal for admitting an additional supply of air in very calm weather. The large lamp is provided with a chimney, which delivers the products of combustion into the shaft of the cowl; this chimney is provided with a regulating damper and one of Faraday's cones for dispersing any down draughts that may possibly occur, and which would otherwise impair the steady burning of the lamp. The dioptric apparatus is octagonal, having 8 panels of refractors, with panels of totally reflecting prisms above and below them. Flashes of red light are emitted from the apparatus at intervals of 45 seconds, the duration of each flash being about 7 seconds and that of each eclipse about 38 seconds. The light is coloured by a chimney of ruby glass on the large central lamp. The intensity of the flashes from this apparatus with the improved lamp of the first order of the Trinity House having 6 wicks, if white light, is about 125,977 candles, or about 37 times the intensity of the originally proposed catoptric light, but in colouring about 70 per cent. of the power is lost; the intensity of the red flashes is thus about 37,793 candles.

Adverting to these improved lamps, one of which we have before us, although at the Great Basses it is consuming the local cocoanut oil, which is found to be nearly equal in illuminating power to the best colza or mineral oil, the lamp is capable, with a very simple adjustment, of burning efficiently all the oils used as lighthouse illuminants, whether animal, vegetable, or mineral. Improvements lately made to this lamp have increased the illuminating power 22 per cent.; at the same time the consumption of oil has been reduced 17 per cent. Within the last five years the power of the first order Trinity House lamp has been raised from 280 to 722 standard candles. By a simple device, the

lamp is made to burn at half or full power, without any alteration in the form or dimensions of the flame, but with a corresponding consumption of oil and intensity of light. The half power is adopted when the atmosphere is perfectly clear, and the full power whenever the transparency of the atmosphere is impaired by mist, rain, snow, or fog. It is found that, with this system, and given a certain quantity of oil to be consumed annually, about 64 per cent. more light is available for the mariner when the transparency of the atmosphere is impaired than if the lamp were burned regularly at a uniform power.

A fog bell weighing 7 cwt., for a signal during foggy weather, is fixed on the outside of the tower, and sounded by machinery fixed in the lantern.

At the erection of the Eddystone lighthouse small sailing vessels were used by Smeaton to convey the materials from the workyard to the rock, a distance of 14 miles, and to land it thereat, and in the case of more recent structures the material has been conveyed in barges towed by a steam-tug. It is evident, however, that with works like the Basses, at distances of 80 and 100 miles from the workyard, such methods of transport would be neither certain nor safe. Two iron twin screw steamers were designed and constructed for the purpose, each capable of carrying 120 tons of cargo. For the purpose of landing and hoisting the material of the lighthouse rapidly at the rock with a minimum number of workmen, the steamers were each fitted with two steam winches, by which the blocks of stone were hoisted on board, stowed below, hoisted again to the deck and from thence to the rock.

On the 27th February, 1870, the first of the steams vessels, the "Arrow," arrived at Galle viâ the Suez Canal, and on the 7th of March following, the first landing was effected on the Great Basses. On the 17th of the same month the light vessel arrived at Galle from London, viâ the Cape, and on the 30th she was moored off the Great Basses, and at sunset the light was exhibited. The workmen were now quartered on board the light vessel, and the "Arrow" was available for obtaining supplies from Galle without delaying the work. The working season ended on the 3rd of

May, when 36 landings had been effected and 220 hours worked on the rock. In this time a dam had been built around the site for the Tower, for the protection of the workmen, the foundation pit had been nearly completed, and part of the landing platform had been built with the stone excavated.

On the 19th October, 1870, the second steamer, the "Hercules," arrived at Galle from London, to co-operate with the "Arrow," in conveying material from Galle to the rock. The first landing for the season was effected on the 28th November, and the last on the 28th of the following April; 84 landings were effected and 651 hours worked. The first stone was laid on the 28th December, after which both vessels were used alternately in conveying the material from Galle to the rock. During the monsoon the foundation pit had been completed and the work raised to the 21st course.

For landing the stones at the rock a strong mast 45 feet long was shipped into the rock and further supported by chain guys: from the mast was suspended a strong derrick which was traversed towards the tower by a winch and chain guy. The steamer was moored with one anchor ahead and a warp to the mooring for veering into position. Two large coir hawsers were abreast to the mooring buoys and two to the rock. The holds of the steamers were fitted with two tiers of rollers, on which the stones were placed; they were brought speedily under the hatchway, where an iron cage working in slides was fitted for conveying them to the level of the deck. One barrel of the forward steam winch lifted the stone to the deck and deposited it on rollers in readiness to go out of the gangway. The shore purchase consisted of three parts of half-inch chain, one end of which was attached to a barrel of the aft winch; from thence it passed through a leading block at the gangway, thence through another leading block at the foot of the shore derrick, thence through another block at the head of the derrick, thence through a block attached to the stone to be landed, and thence to the head of the derrick, where the end was made fast. A strong chain was also attached to the stone and No. 2 barrel of the forward winch, on which was a powerful brake. To land a stone, the aft steam-winch was put in motion, and as the stone went over the side

the guy chain was eased away until the stone entered the water; it was then gradually checked and "paid" away at about the same speed as the aft winch worked the stone ashore. When the stone reached the rock and was high enough, the derrick was swung towards the tower by means of the small winch on shore, and the stone lowered.

This is the first known instance of a vessel landing heavy material in a seaway by her own steam power, excepting where the load could be deposited by her own derrick. Although necessary, owing to the shallow water, to moor the steamers at a distance of 30 fathoms from the rock, stones weighing on an average 2½ tons were hoisted out of the hold, landed, and deposited 28 feet above the surface of the rock at the rate of 10 per hour. As soon as the cylindrical base of the building was completed, a steam winch was fixed upon the rock for hoisting the stones to the top of the work.

On the 16th November the first landing of the third season was effected; the last was effected on the 2nd of the following May. During the season 74 landings were effected and 679 hours worked. The number of working days of 10 hours from the first landing on the rock to the end of this season was 153 days, 3 hours, and from laying the first stone of the tower to setting the last stone only 110 days. The light was exhibited on the 10th March, 1873, and has been continued with regularity every night from sunset to sunrise. There are two European and four native lightkeepers attached to the lighthouse, who are relieved monthly, or as nearly so as practicable. One European and three native keepers are always at the lighthouse, while one European and one native are on leave at Galle.

This important work has been executed in a tropical climate, at a distance of nearly 7000 miles from this country, by a small band of trained Europeans aided by natives, and under exceptional difficulties, without loss of life or limb to any person employed.

The total cost of the work, including all incidental expenses, was about £62,000, being within the estimate for the work.

It might fairly be assumed that such a work as that now on the Great Basses, fitted with all the modern improvements in en-

gineering and optical science, would have proved exceptionally costly compared with similar works in this country, but such is not the case. When it is stated that, per cubic foot, the work has been executed at less than half the cost of the Eddystone, I think that no better proof could be offered of the advantages now derived by a judicious application of scientific improvements to constructive works.

The success which attended the execution of this work encouraged the Board of Trade to order the erection of a similar lighthouse on the Little Basses. This work is of even greater difficulty than that at the Great Basses, the reef being 20 miles farther from Galle and awash at low water.

Mr. William Douglass, who erected the Great Basses lighthouse, has been entrusted by the Trinity House with the work, which was commenced in November, 1873.

On the 28th February last the first stone of the lighthouse was laid; the solid portion of the tower is now completed, and it is expected that the light will be exhibited in another year. The tower, of which a diagram and model are before you, is of Dalbeatee granite; it has, like that for the Great Basses, been prepared and fitted together in this country and delivered at Galle. The tower is 33′ 3″ diameter at the base and 16′ 6″ diameter at the top under the gallery. The shaft is a concave elliptic frustrum, the generating curve of which has a major axis of 148½ feet and a minor axis of 28 feet. It contains about 1720 tons of granite. The internal arrangements and fittings are very similar to those in the Great Basses.

With two rock lighthouses so near each other, it was not only necessary that there should be a clear and unmistakable distinction between the two lights, but also that the two lighthouses should be quite distinctive by day. To effect this, the lantern of the Little Basses was provided with a domed roof, the roof of the Little Basses being conical; and further, the Little Basses has a second gallery 10 feet below the lantern gallery.

A light of a novel and very distinctive character has been adopted for this lighthouse. It belongs to a class of lights called "Group Flashing," which have for some time received the attention of the Trinity House.

The system consists in giving two or more flashes, in quick succession, at intervals of say ¼, ½ or 1 minute instead of a single flash at the same intervals.

The first light established on this system was a floating catoptric light at the "Royal Sovereign" shoal near Beachy Head; this light gives 3 flashes in quick succession at intervals of one minute. It has been found to be very distinctive and efficient.

The first order dioptric light for the Little Basses has been designed by Dr. Hopkinson and manufactured by Messrs. Chance Bros. and Co. It is intended to give 2 flashes in quick succession at intervals of one minute: this is accomplished by cutting away a portion from the opposite side of each pair of lenses so as to bring their axes closer together, and to the required angle in azimuth for the short eclipse between the flashes, as will be readily understood from the diagram.

The maximum intensity of this light, with a six-wick improved lamp in focus, is about 88,900 candles, being about 1234 times the intensity of the beam originally emitted from the Eddystone. We have here, lent by the Trinity House, one of the first lenses used by them in an English lighthouse; this was at the Portland Lower Light in 1786, being 33 years before the invention by that eminent Frenchman Augustin Fresnel of his Lenticular Lighthouse System. In 1836, at the Start Point Lighthouse, on the Coast of South Devon, the Trinity House installed the first dioptric apparatus on Fresnel's System in an English lighthouse. One of these lenses we have also before us; there were 8 lenses to the circle, with fixed silvered mirrors above them. The intensity of the flashes of this light with its concentric four-wick lamp was about 49,392 candles. This apparatus was manufactured by Messrs. Cookson and Co., of Newcastle-on-Tyne. It is exceedingly interesting and instructive to compare the imperfect material and workmanship of these instruments with the excellent apparatus now manufactured. The illuminating apparatus at the Start Lighthouse, and that of the South Stack Lighthouse near Holyhead, have lately been replaced by dioptric apparatus having six lenses to the circle. These apparatus have been manufactured by Messrs Chance Bros. and Co. The intensity of

their flashes with their six-wick lamp in focus is about 169,360 candles, or about twice the power of the light of the Little Basses. These two lights are at present the most powerful oil lights in the world.

Before concluding, it may be well to make a few remarks on the two other sources of lighthouse illumination adopted—viz., coal gas and the electric light. The latter we owe to the discovery of Faraday and the ingenuity and inventive genius of Holmes. Neither of these illuminants is applicable to the class of lighthouses which are the subject of our lecture, owing to the want of space for the requisite apparatus and fuel. Both systems are, however, on their trial in this country, and at present their extra cost appears to be the only barrier to their more general adoption for shore stations.

At the Haisboro' Lighthouses on the coast of Norfolk, coal gas on the system of Mr. Wigham, of Dublin, has been under trial about four years, with satisfactory results. Several of these lights have been established on the coast of Ireland by the Commissioners of Irish Lights.

The electric light has been under trial in this country nearly 18 years. It was first tried at the South Foreland High Lighthouse in 1858. There are now two electric fixed lights at the South Foreland, and an electric revolving light has been exhibited at the Souter Point Lighthouse on the coast of Durham for the last 5½ years. The flash of this light has an intensity of about 392,000 candles, or about 2¼ times the intensity of the oil lights at the Start and South Stack. With the improved electric machines of Gramme or Siemens, which are to be seen in the Loan Exhibition, this enormous intensity of light would probably be increased 5 to 6 times; indeed there appears to be no practical limit to the power available for the service of the mariner from this marvellous source of illumination. Unfortunately, such fogs occasionally occur as to eclipse even the sun; consequently, the most powerful artificial light must be unable to cope with them. Recent investigations of the Trinity House, aided by their scientific adviser Dr. Tyndall, whose researches on fog are so well known, are leading to the development of powerful sound

signals as aids to the mariner at such times as light is no longer serviceable. Some of these instruments have been lent by the Trinity House, and are to be seen in the lighthouse section of the Loan Exhibition.

The CHAIRMAN :—Perhaps there are some of you who might wish to put a question or two to Mr. Douglass, who I am sure will be glad to add anything in elucidation of what he has been explaining to us. But if that is not the case I will ask you to join with me in thanking him for the very able and capital lecture which he has given us this evening. I am sure you have all listened to it with a great deal of pleasure, and with great attention ; and I am not surprised that you have done so, because, as I told you at the beginning, I believe there are few people in the world so capable of explaining to you what a lighthouse is as Mr. Douglass. He has been employed throughout the whole of his lifetime in these structures, and when you consider the privations and labour he must have undergone I am sure that you will join with me in giving him that applause which he ought to receive. These labours have led to his receiving the appointment of engineer to the corporation of Trinity House, and in the course of those duties which he has performed so efficiently he has improved our lights and lighthouse establishments to that extent that they are now, which he has described to you, although he has not mentioned that he had a great deal to do with it. Still it is the case that it is owing to him in a great measure that our lighthouses are so efficient and so good. I therefore ask you to return Mr. Douglass your hearty thanks for the lecture he has given us.

WHAT IS A CRYSTAL?

By N. Story Maskelyne, M.A., F.R.S., etc.

July 25th, 1876.

Macleod of Macleod in the Chair.

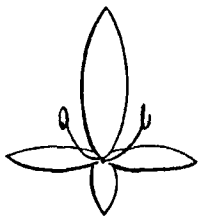
The Chairman: Ladies and Gentlemen,—On behalf of the Lords of Committee of Council on Education, I have the honour to introduce to you Professor Maskelyne, who has been so good as to undertake to deliver a lecture here this evening on the subject of What is a Crystal?

Professor Maskelyne: I believe I have undertaken rather a rash task in hoping to answer this question in the short space of an hour. In fact it would be quite impossible for me to do so in a complete manner; but I shall endeavour to inform these mute models and specimens, which I have selected from this Exhibition, with so much of life and power of language, as may enable them to answer this question—answer it, that is to say, not indeed completely, but sufficiently to give a sound idea to anybody who, without wishing to go into the more intricate esoteric questions of the science, would yet like to know generally what is the view taken by crystallographers of that wonderful little object which it is their business to endeavour to elucidate, viz., a crystal. Of course, if I do not succeed in drawing an answer from these little models, it will be my fault, not theirs; but first of all I will try to extract one from my hearers. I would ask each of you what meaning, in your own thoughts, do you attach to the term "crystal"? Probably you have gone into druggists' shops and have seen a large number of bottles full of crystalline substances. You have, perhaps, looked at the beautiful little

crystals that now and then you find in the sugar-basin for use in your coffee, and have admired, no doubt, the wonderful geometry of those little crystals; or, again, you have looked at the salt that sometimes comes to our tables in exquisite little cubes, and you have asked yourselves, it may be, or perhaps you have not, what is the marvellous power which has thus caused these substances so to fashion themselves in this systematic way. Now, if I do not hope to get in this way an answer to our question, I shall ask you still to consider what is the impression left on your minds on looking on the crystals I have mentioned, or, if you please, on these crystals that come up in the form of hard little minerals from the depths of the mine, or these again, which constitute the ingredients of the very rock through which the shaft is driven by which they come. Now your answer to the question would most likely be a vague one; you would probably say you are aware that each of these particular substances presents itself under a peculiar form; you will, for instance, have observed that certain of these substances present themselves generally under forms that are more or less needle-shaped; that they are little, long, bright, flat, needle-formed crystals. That is one experience. Others again, you will remember, are not of that kind, but are more or less tabular and flat, or, it may be, like a cube, not particularly elongated in any one direction. In other words, you see that there is an infinite variety amongst these crystals, and that each particular substance has its own habit of growth, if I may say so, into a crystal. But while you will have observed that there is this large variety in the character of the crystal, there is one thing you cannot have observed, because it is only the result of long and laborious induction, and that is this, that every one of these crystals, varied as they may be, is formed in accordance with a strict geometrical law. I am not going to-night to begin with that law, or to make it the chief subject of this lecture, for it would take up the greater part of my lecture to completely explain it; but I hope to illustrate that law by giving you some of its results, and also to show you how the geometry of the crystal really hangs on that single law as this cluster of crystals hangs upon that thread.

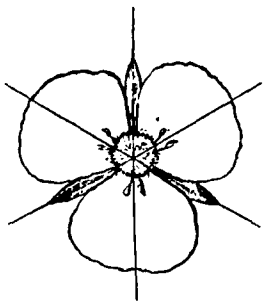
I shall make certain statements which you must accept from me. One is that all these crystals, whatever they may be, present one cardinal feature, and that cardinal feature is symmetry. Now what is symmetry? And in what sense do we use that word when speaking of crystals?

FIG. 1.



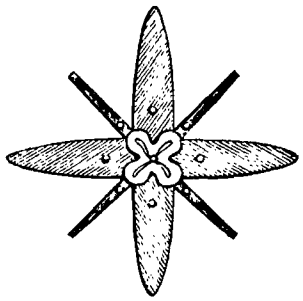
Veronica.

FIG. 2.



Alisma.

FIG. 3.



Paris quadrifolia.

Symmetry, you may say, is the very soul of the forms of nature: it is the very essence of the beauty of most works of art. Take, for instance, architecture as an art; take plants, if you please, from the domain of nature, or animals, and ask yourselves are they not all replete with symmetry. I have put on the wall two diagrams with flowers upon them, because they will illustrate one or two points I have to speak of. There you will see you have symmetry, for I have drawn lines through one of these flowers, which lines may, perhaps, strike you as even belonging to the flower, but such are really lines with respect to which those flowers are symmetrical. The proof that we have symmetry in this case is that each element of form in the flower is repeated on either side of one or more such lines; the two halves of a petal for instance. Thus we mean by symmetry in nature or in art that a certain element of form is regularly repeated. And the human mind is so constituted that one of the ways in which natural objects appeal to and awaken pleasure in it is by the symmetry inherent in them: symmetry which is the repetition of morphological or

form-elements according to a law, the law depending on the nature of the case. You will find, for instance, that there are laws of symmetry in botany which are impossible as laws of symmetry in crystals, and so on. In botany you are not able to state why, for instance, a flower presents you with pentagonal symmetry, that is, with 5 petals, and five of each of its other elements of form; or with trigonal or hexagonal symmetry, that is, with 3 or 6 petals. You cannot tell the reason why that takes place in botany, because we do not know the fundamental geometrical influence which is at the root of the development of the parts of a plant. There is no doubt some subtle geometry at work that we have not yet fathomed; but in the crystal we have so far done this that we know the fundamental law which regulates each face, edge, and angle of the crystal. We know that fundamental law, and we can now assert, as a deduction from that law, that only certain kinds of symmetry, which we had previously recognised by observation, are possible in crystals.

I should like just to consider with you a few kinds of symmetry, that we may understand better what we mean by the term as applied to a geometrical structure. We will go to the art I spoke of—the very essence of which is symmetry—architecture. For instance, in the case of a tower, the architect invariably, if he makes a square tower, will add ornaments so that they may be repeated symmetrically with the tower itself—his pinnacles, his turrets, his windows, everything is more or less in accordance with the symmetry of his tower. He may perhaps sport as it were a little with that symmetry. He may throw out here and there some accidental little feature that does not look as if it belonged to the symmetry, but the very spirit of his art will consist in partially concealing the fact that all that time he is really obeying the law he has laid down for himself. Now what I am going to say about towers and spires has a very direct bearing indeed upon the subject of crystals. If you please, as being more familiar objects, let us consider what sort of towers and spires we can conceive. You may imagine a tower like the Victoria Tower, or like the clock tower, with a square base; that is if you cut sections across them everywhere, these are

always either square with similar ornamentation upon the angles or sides of the square; or you may have such a tower with a spire upon it, a square tower with a pyramidal spire. Again you may have a trigonal tower, a tower with an equilateral triangle as a section; such as Stourton Tower in Somersetshire. The section of that tower is an equilateral triangle. Or it may be a hexagonal tower, such as you have in the tower of St. Dunstan's, Fleet Street. Or you may select a tower like that of St. Mary le Strand, of which the section is a rectangle; the angles are right angles, but two of the sides are longer than, and are different in the treatment of their ornamentation from, the other two. These are among the kinds of towers we can suppose, and on each of these a spire corresponding to it, which may be as high as you like or as low as you like. The architect can choose the height as he pleases. Here (see fig. 16) is a square with certain ornaments on it, lines crossing it, and the angles cut off by these little wires, and that will represent fairly well the base of one of our towers. If you imagine the tower to be standing on a sheet of water, and that you are looking at it from the front, you will see the tower reflected on the sheet of water as if there were another tower standing with its point downwards with the base of the tower meeting the one you are looking at. The sheet of water in that case reflects, like the surface of a glass mirror in

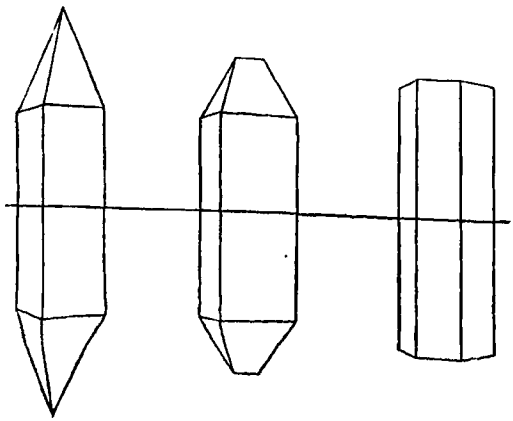


FIG. 4. FIG. 5. FIG. 6.

which you see the upper half of the picture repeated, and that is exactly what I mean when I speak of a *plane of symmetry*. It divides the figure so that the upper and lower halves are symmetrical; that is to say, the upper half corresponds exactly with the lower half, and if you draw lines perpendicular to that sheet of water, lines we will say 50 feet above and 50 feet below, those lines would meet points which would correspond above and below. That is the definition of a plane of symmetry. In that case the sheet of water would be the plane of symmetry parallel to the base of our tower.

Now if I take a crystal of this kind in which you see we have two spires with bases of six equal sides, and a regular hexagon, for section; below and between the two there is this prism, which is also hexagonal; if we were to cut this in half, the upper and lower half would exactly correspond to each other, and the section which divided them would be the plane of symmetry dividing that crystal in two. If again I cut the crystal down in certain directions, perpendicular to this plane of symmetry, there will be six such sectional planes by which I can divide my crystal symmetrically; that is to say, the two halves, in each case, will be, the one to the other, as the reflection would be to an object seen in a mirror, just as in the case of the symmetry of a tower in regard to a sheet of water.

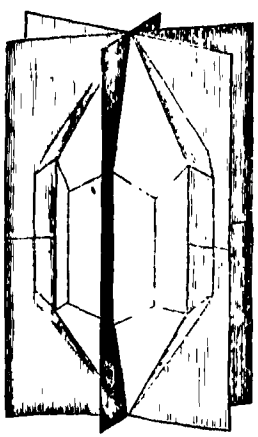


FIG. 7.

In describing the architecture of a crystal we have to change the word tower into prism, and we may speak of a prism as a rectangular prism, a triangular prism, a hexagonal prism, etc., according to the character of its cross section: and instead of a tower supporting a spire, we will now speak of a prism terminated by pyramids. Now we may have prisms on crystals of the following kinds. First let the crystals stand upright —perpendicularly—on their bases. Then the cross section may be square, or rectangular, and may have its corners cut off by planes

represented in section as lines making it into an octagonal figure with *equal angles*. Or planes may cut off its corners at angles which are different on adjacent sides (see figs. 15 and 16). Or the section may be triangular, and the angles of the triangle may be cut off by planes, *i.e.*, by lines representing planes which make hexagonal, equiangular figures, and so on. But no section of a crystal can be an equiangular pentagon. Or, secondly, we may have crystals which when placed on their base, or on a cross section parallel to their base, would lean over to one side. Some such crystals can be divided symmetrically by a plane; some cannot. But in the last case there is still to be found a centre of symmetry, because for every point on one side you can find a corresponding point at an equal distance on the other side along any straight line passing through this centre; so that there is a centre of symmetry. Consider next a crystal prism with pyramids terminating it, and you will see when speaking of this as a crystal symmetrical to an axis, which is the next term I am going to use, what meaning I attach to the words tetragonal, hexagonal, and so on; that it is tetragonal when it comes into four *positions of similar aspect* when turned once round the axis; or trigonal when it has three, or hexagonal when it has six such positions of repeated identity. So we have now a particular meaning for that expression. Take for instance this crystal, fig. 4 or fig. 8; if I turn it round its axis through 90 degrees, that is a quarter of a circle; its aspect is the same. Turn it again, you see it is exactly what it was before, and I can do that 4 times before the figure comes back to the position which it started from. That is a tetragonal prism. We can do the same thing with other figures. We can do it 3 or 6 times round a trigonal

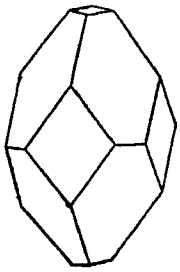


FIG. 8.

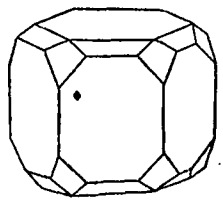


FIG. 9.

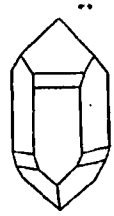


FIG. 10.

or hexagonal axis (see figs. 10 and 9). If we examine these different crystals you will see that we are able to get out of them all the sorts of symmetry that I spoke of just now; but the point I want to call your attention to is that in crystals we have not the advantage of being able always to refer a crystal to the particular kind of symmetry that it represents simply by looking at it. We have to study it to a certain degree, to bring a certain amount of crystallographic knowledge to bear, to determine whether or not that crystal presents one or another kind of symmetry. Now let us look closer into what we mean by this question of symmetry in the crystal. Take the trigonal case, and you see if I turn this quartz crystal through the sixth part of a circle I bring my pyramid into a position similar to that which it was in at first: but the relative situations and aspects of the faces of the crystal in the three positions, though a similarity is recognisable in them, are not strictly identical. The question is in what manner can you say that that sort of symmetry exists in this crystal, because when you come to examine it you find this awkward fact about it, that the faces that should be repeated symmetrically (morphological elements, in fact), are not all nor any of the same size. Of course, in a tower the four sides are of the same size, and so are the sides of a spire; in fact, the very essence of symmetry in a tower or spire you would say consisted in the sides being comparable in this respect. In a crystal that is very rarely the case; in point of fact it is quite an exceptional thing to find a crystal in which the faces that ought to correspond to one another are exactly of the same size. Sometimes you have a crystal compressed and flattened along one direction, and a plane which, when you come to look at it, you would say ought to be a large plane—is a small one, whilst others are large which ought to be small. You can hardly, for instance, see them sometimes. Here is a crystal in which the green spots on the pyramid represent one set of faces, and the red spots represent another set. Here is a large face with a green spot, and there is a very small one.

But now I come to a statement which I must ask you to accept as the result of experience. Although we find the faces of crystals vary in this manner, we always find *the angles between the faces*

that correspond to be the same. That is the important fact out of which crystallography took birth. It was the establishment of this fact as a certainty that made it possible for us to create a science of crystallography, and upon that fact the science has steadily grown. Now observe one result of this fact, that though the faces of the crystal may present any size, big or little, yet they do not vary in *relative direction;* that is to say that faces to be symmetrically repeated must be inclined at the same inclinations on other faces which correspond to one another symmetrically; and that, independently of their relative magnitudes. We will just give a name to that sort of thing because it is always well to have simple terms to use. A crystallographer when he takes a crystal, such as this one of Apophyllite, looks upon it, and selects certain faces. He says this one and this one and that one and that one are evidently four faces that correspond to one another; and there are four more at the bottom here. They are the four faces repeated four times above and four times below this plane of symmetry, but some of these are large and some are small. How do we know that these faces are really the faces that correspond to one another? That is a more difficult thing perhaps to explain in a moment, but in the first place their physical characters are all alike, and are very much unlike the physical characters of the other faces of the crystal. For instance, these faces here are deeply grooved in one direction, all of them. The little face at the top is covered with myriads of little pyramids, and is unique until you turn it over, and then the parallel face at the under side is found to have physical characters exactly like it. Again, these faces which I first spoke of are faces which carry a singular kind of glistening lustre which is quite peculiar to them. One can see at once that they correspond to one another; but it is not always that we are able thus easily to say that this face corresponds to that, and that we may put all these faces together into a group of symmetrical faces, in fact into what the crystallographer calls a "form." It is not always we are able thus to assert that these faces belong to a "form." We have sometimes, in order to assert that, to go into careful measurements, to take an instrument called a goniometer, an

instrument for measuring these angles, and determining the angles which these faces make with the faces that they are in contact with, and then when we have measured those angles and have found that the corresponding angles here are exactly the same as the corresponding angles there, then we are able to say that these faces do really belong each and all of them to the same *form*. That is how we come to recognise symmetry in a crystal. The faces need not necessarily be all of the same size, but they are related in the same way, and inclined at similar angles to the faces around them, and they also present the same physical characters; or we may say, that faces the directions perpendicular to which in the crystal are the same in their physical characters, such faces and only such faces of a crystal are those which are repeated symmetrically. Symmetry in a crystal therefore consists in this, that similar faces on it are repeated in obedience to a law of symmetry, and when I say that, I mean you are to identify these faces by the fact, first, that their angles of inclination correspond, and secondly, that their physical characters are the same; that the physical properties which the crystal presents perpendicularly to these faces as regards heat, light, friction, electricity, elasticity, cohesion, every agency in short, by which you can solicit them, are the same, and, I add to that, these physical characters are different from those which characterise the directions perpendicular to any other faces of the crystal. There we have given us a true definition of crystal symmetry. If you consider what I have said, that the real identification of a plane or face upon a crystal involves a certain power on your part of identifying the physical characters of the crystal in a direction perpendicular to the face, you see that it almost stands to reason, without further recourse to experiment, that wherever you get a section through a crystal parallel to the given face that section must have characters similar to those of the face; at least, so far as that wherever you draw a plane through a crystal parallel to a face, the section you make must present the same symmetry as the face presents; that is to say, you cannot cut a plane parallel to the face of a crystal presenting tetragonal symmetry, which instead of being symmetrical in this way should present trigonal symmetry. That is a very

important matter, because what we practically have to ask now is what kind of sections *can* we cut through crystals—in what sort of ways can you cut sections in any crystal so as to exhibit and define its symmetry, and what sorts of symmetry can we thus reveal?

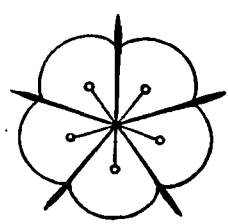
First of all, we will speak from experience. If you come to look into the question and study a number of crystals, that fact which I mentioned at the beginning is very striking, viz., how many crystals are more or less prismatic in character, long needle-shaped, tower-like? In fact, they are in the majority; when you come to inspect these crystals you find that their prism sections belong to one or other of the kinds that have been defined; they may be tetragonal, or trigonal, or hexagonal, or they may be rectangular or lozenge formed (the symmetry in these two cases being identical in character). And if we carry sections through the crystal in any other directions parallel to faces, we further see that, besides these sorts of symmetry, the section, like the face itself, may be symmetrical only to a centre, or may have no symmetry at all. Now the crystal faces, and consequently the crystal sections that may be cut parallel to faces, which belong to the lower types of symmetry, are by far the most frequent on a crystal; those of higher symmetrical type being either planes of symmetry themselves or perpendicular to axes of symmetry, and so to two or more planes of symmetry.

Thus in the first place, except in one variety of symmetry, or system of crystals, you will never have more than one direction in a crystal, the section across which resembles a square or is trigonal or is hexagonal; that is to say, you may have a crystal of this kind (figs. 7 and 9), with its hexagonal section perpendicular to an axis of form—the *morphological axis*, the direction of which is now vertical. You may have that, but with the exception of one single system, as we call it, you cannot have more than one such section as that.

In the same way with the square section. There is only one kind of crystal-symmetry in which you can have more than one square section. That one kind in which you can have more than one square section is the same as the kind in which you can have one other kind

of trigonal—not hexagonal, but trigonal section; and it is a very curious thing that this should be so; you may prove the necessity for this geometrically, but it is not the less remarkable that the only exception to the impossibility of your having crystals with more than one square or triangular section is this, the cubic system. (See fig. 12). That is the system of which you see a very complex form that has been built up within this wire model. Here again is a solid model with a series of planes, representing the possible planes of a crystal which also belongs to that system. It is a very complex system—the most complex of all—and the system, at the same time, to which the simplest figures which we know belong. Three of the platonic solids—the cube, the octahedron, and the tetrahedron—all belong to that system; on the other hand the other platonic solids, the dodecahedron and the icosahedron, are impossible in that crystal or on any other, because they present sections which are pentagonal; they are like the section of this flower, the *anagallis*, and that is a kind of symmetry which is impossible in a crystal.

FIG. 11.



Anagallis.

Now we have considered certain kinds of crystals; we have seen that only a limited number of kinds of prisms or pyramids, or indeed of other solid forms, can exist in crystals. I have told you that experience first asserted that, but that now theory makes it certain. Then we have seen that the trigonal and tetragonal kinds of symmetry can only exist uniquely—that you cannot have them combined in the same crystal except in that one single case, the cubic system.

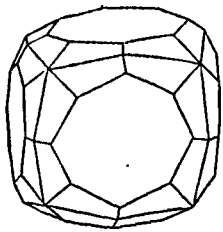


FIG. 12.

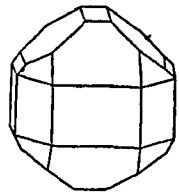


FIG. 13.

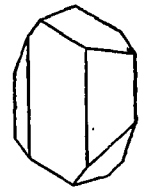


FIG. 14.

To the different kinds of symmetry that are thus possible, the crystallographer has given names; and has divided them into six different classes. There is that most complex one in which the square and trigonal kinds of symmetry are united together in the same figure; that is the first, which we call the cubic system (see fig. 12). Then we may have the kind with a tetragonal base like this, but where the axis of rotation for revolving the crystal round into its four positions of identical phase, is longer or shorter than either the sides or diagonals of the base itself (fig. 8). If on the other hand, this axis was of the same dimensions as the width and depth of the base, this crystal would have been an ordinary cubo-octahedron, and that would have been a combination of figures belonging to the cubic system. This system, then, which has one square section we call the tetragonal system. We have a system which has one hexagonal section and that we call the hexagonal system (see fig. 9). To that system belongs a peculiar variety of symmetry, presenting a certain defalcation in the faces which results in a trigonal disposition of the features of the crystal, as in fig. 10; but so that these two, the trigonal and hexagonal, practically belong to the same system. We have, therefore, the cubic, the tetragonal, and the hexagonal systems. Those are three of the six systems.

Now we come to another. That is the system I have here (see fig. 13), and I must say a few words just to show how the angles come in to help us. This represents a crystal in which the lengths from side to side, from front to back, and again from end to end, represent three different magnitudes; at the same time that the crystal is symmetrical to three planes perpendicular to each

other, which we may suppose to pass through the middle of the solid; three planes which, perpendicular to each other, divide the crystal symmetrically. I say the three dimensions of the crystal *represent* three magnitudes. Yet we have seen that relative magnitude in length, breadth, or thickness has no place in crystallography, but only the magnitudes represented by angular inclinations.

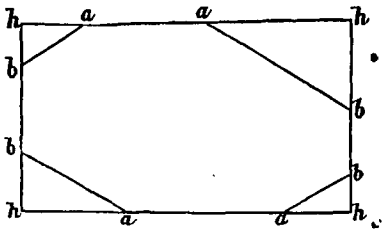


FIG. 15.

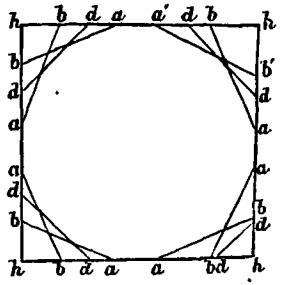


FIG. 16.

Let me explain this. Here is a square, and here a rectangle; crystallographically they are equivalent figures. But if an angle of what we may call a crystallographic square is replaced by a line representing the trace of a crystal face, the angles made by the line with the two sides of the square are equal; or, if they are not, there must be two lines, *i.e.*, two faces similarly inclined on the sides, as in this model (fig. 16). In a crystallographic rectangle this is not the case. In the case of the square with each angle cut off by a crystallographic line, the lengths of the parts of the two sides cut off by the line are obviously equal; in the case of a crystallographic rectangle they cannot be equal (fig. 15). We say then that in the first case it is possible, in the second case it is impossible to represent these lengths by equal magnitudes.

The sort of symmetry then, in which there are three or more crystallographically rectangular, but no square sections, furnishes us with a fourth system. We have the cubic, the tetragonal, the the hexagonal, and this fourth system with three perpendicular planes of symmetry, namely, the orthorhombic, or upright rhombic system; it is called the rhombic system, because the sides of the prism-section are parallel to the diagonals of the

rectangular prism-section, and form a lozenge, which is called by mathematicians a rhomb. Another name given to it is the prismatic system. Next we have another kind of crystal, of which this is an illustration, a figure which does not stand perpendicular on its base but heels over (see fig. 14). There is one plane of symmetry passing through the crystal, and it is therefore called the mono-symmetrical system by Groth, but we generally call it the inclined rhombic or clino-rhombic, or the oblique system. Finally we have the system in which you have no plane of symmetry at all;

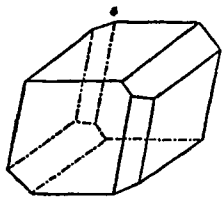


FIG. 17.

but the only kind of symmetry which the crystal presents is symmetry to a centre, and that is the asymmetrical or the anorthic system (see fig. 17). Those are the six systems, and in the sketch I have given you of them I have illustrated the general principle ruling the arrangement of crystals according to their symmetry.

Let us go now to another subject. I said just now that we know mathematically, and as a recent result of mathematics, that those systems are the only possible systems; and we know that, because we know by experience that what is called the fundamental law of crystallography is true. We have not yet said anything about that law, and if you please, we will not just at present, but let us consider for a moment something else, and that is the effect of heat upon a crystal, or the behaviour of a crystal in restricting the motions of heat or light, or other forms of energy. What is the course of heat when it enters a crystal? Heat, of course, the ordinary radiant heat is nothing else but light, that is to say, heat and light are bound up together. They are like threads of the same cord and you cannot separate them. The

radiant heat and light, or radiation, when it approaches and enters a crystal undergoes in it extraordinary changes.

In fact, when one comes to deal with the more intricate questions of crystallography, we have to recognise that a crystal is a molecular system, the distribution of the molecules in which is the thing we have to unravel. Consider the change effected in the course and fashion of a ray of light when it enters a crystal. You know at once the change is profound, that it varies with the direction in the crystal, that as a rule (not without exceptions, and exceptions that are always explicable and beautifully regular), but as a rule the radiant heat and light is broken up when it enters the crystal into two rays which can only vibrate in certain directions; that is to say, that the luminiferous ether by the means of which we explain all the ordinary phenomena of light undergoes, by the influence of the molecules with which it is hampered, a certain kind of constraint in the transmission of the light-thrill or vibration, which, as it traverses this ether, has special modifications impressed on it, and is only able to follow certain paths within the crystal, which paths will depend on the direction of the light, and on the construction of the crystal in which the light movement is propagated.

But now let us pass from this more complex question to one, the result of which is somewhat more simple. Let us take what happens when heat begins to become sensible heat, when the crystal gets warm. What does that mean? It means that some portion of this vibratory movement has been converted by the crystal; that there has been an absorption, that the motion that before was confined only to the ether and that would otherwise have passed on, has been connecting itself with the little molecular systems that have hitherto been hampering this ether, and that these molecular systems are now vibrating more vividly, more strongly, and that the excursions that they are making—for remember no crystal is without heat—no body in nature is without heat, and is therefore not without motion—therefore I say the motions of these molecules have been amplified by the addition of the heat to them and now the crystal begins to undergo that change which matter undergoes when its temperature is raised—it

begins to expand; but the expansion does not take place equally in every direction. It takes place much more in some directions than in others. The only thing you can assert about it *a priori* is this, that it will take place equally along all those directions which may be symmetrically repeated in the crystal. Along all these directions the increase due to the action of the heat will be the same, and it will be different along all other directions; that is to say, the crystal will expand along those directions more or less than it will along any others, and the result of that will be that the angles of the crystal will as a general rule change, and as the angles change we can measure on the goniometer the kind of change that is taking place on the form of the crystal. This marvellous fact is of a piece with all the other facts we know about crystals. Let us now come to some explanation of this. I find in the collection here a very interesting and remarkable little model that illustrates a mathematical theory of the molecular construction of a crystal. It had a long name that a good deal puzzled the translators for the guide, but the meaning of it is a Space-trellice-work, or "Trellice work in space," a reticulation in three dimensions; but it will serve to illustrate very beautifully a simple theory of the molecular construction of a crystal. If you suppose a crystal to be, as we know it is, a molecular structure; then you have only to imagine that the centres of mass of the different molecules are ranged along in straight lines—that if you take a line passing through any two of these you will find if you go on, to the same distance again in this model, that you would come to another of these molecules, and so on as far as the crystal extends; that, in short, these little molecules all lie at equal distances along any line you choose to pursue. Now let us suppose an accession of heat to affect these molecules; they are always moving, and as you increase the temperature they move more vigorously. It is difficult to conceive the sort of motions that they must represent, but this exertion increases as the temperature increases, and in directions that are not equivalent, in different degrees, and there you have at once the explanation of the kind of change we spoke of just now. Supposing the motion of these molecules to follow a regular law, and that

for a certain temperature the motions are definite, you may conceive that along any one of these lines all the molecules that are of the same temperature will drive away as it were the molecules next to them until they are stretched out along that line to equal but greater distances, and those distances will be different, from the new distances that will be taken up by the molecules along another direction in the crystal. In short the only directions along which the new intermolecular distances will be the same will be those directions that are symmetrically related to one another, these, namely, that belong to the same set of symmetrically disposed lines. You see then that there we have at any rate a theory—it is only a theory, but it gives us a very complete idea of the way in which heat may operate in producing the effects that have been described to you. But now let me just ask you for a moment to consider what are the conditions in a system of this kind for the existence of a face on a crystal. We have said that the molecules of the crystal, the centres of mass of the molecules, must be at equal distances along any assumed direction in which any two of them lie. If now I only say of a face of a crystal that the necessary condition for the possible existence of a face on a crystal shall be that it contains the centres of mass of three molecules that do not lie in a straight line—that is to say, that lie in different lines, and three points not in a line are enough to give you the position of a plane—then any three such points are sufficient at once to give you the position and the character of the plane that passes through them.

If we assume that as the necessary condition for a face upon a crystal, you see what we can at once assert with regard to the faces of the crystal. We can say that no face can exist in that molecular system that does not contain a certain number—in fact an unlimited number—of these little centres of mass ranged in their regular order. If the plane be perpendicular to a direction around which there is symmetry, then these little centres of mass will be distributed symmetrically round any line of molecules parallel to that direction; and they will be distributed in tetragonal, trigonal, or hexagonal kinds of symmetry. On the other hand you will see also that this plane passing through those different little centres

of molecular mass, will intersect with any other directions along which the molecules were lying, only in a certain way, and that way is this. Supposing we take one of them, and you may take any one you like, for a centre. Just suppose that three lines are selected, such as these three forming the edges of this model as it stands, and we can move them about in any way; supposing that these three rods of the model represent the directions of three lines passing through this angle-point as the centre. Now suppose another face belonging to the crystal, which, as I have defined it, must be a face passing through some three other centres of mass that do not lie all in a line: obviously that plane if continued far enough will meet these edges; even if it is parallel to two of them it must meet the third; and if it is parallel to none it must meet all three.

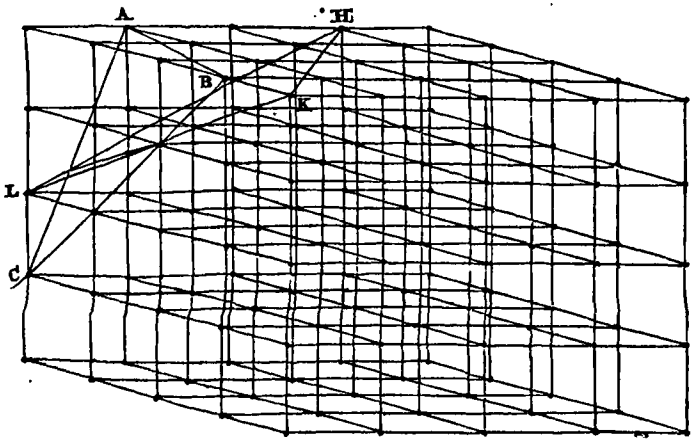


FIG. 18.

If it meets all three, you see that it only meets them in certain numerical multiples of these distances that separate the centres of mass of these molecules. Let us have a name for that. I will call these distances of the molecules on the three chosen lines the parameters. We will call this one L, this other M, and the one in this direction N. Then these molecules along that line

are separated by the distance L, along this direction they are separated by the distance M, along that direction the distance is N. L, M, and N, may be any quantities you like—nature dictates them, we do not—but they vary with the temperature as you have seen. All you can say is that any other face or a plane parallel to a face belonging to this system will, in passing through one of the molecular-centres in each of our axial lines, intersect those axial lines at any distances from the centre which will be integral multiples of L, M or N. Thus the plane bounded by the triangle, A, B, C, meets the lines at the distances L, 3 M, and 4 N; that bounded by the triangle H, K, L, meets them at the distances 3 L, 4 M, 2 N. That is the fundamental law of crystallography. We have worked backwards to it as it were, but I have asserted that the laws of symmetry in crystals are the results flowing out from the geometrical law. I ask you to accept this assertion that they are the natural true mathematical deductions from that law. We have considered then what were these kinds of symmetry, and what is meant by directions being repeated symmetrically in a crystal, and we defined the repetition of a crystallographic direction to be the repetition of directions of similar physical properties. Then we went on to consider what the effect of that would be if you examine crystals by the change they undergo when their temperature is raised, and we passed from that to a hypothesis regarding the molecular structure of the crystal, and endeavoured to conceive of such a molecular structure as would enable us to interpret this phenomenon by a simple law: and the law which came out from this theory not only gave a possible explanation of the changes in question, but accorded also with the law which we had assumed to underlie the relations of the faces of a crystal to one another; therefore you see that if we seem to have reasoned somewhat in a circle, the steps by which we have pursued our path have been, nevertheless, perfectly legitimate. The only thing we have really assumed is this theory, that molecular distances along any given direction are equal. That is the only assumption that we have had to make and I think that is not an assumption of any very great magnitude. I have endeavoured to make these models and

a few crystals speak to you in intelligible language; to sound the notes that should draw out their tones from them. If it be that I have not quite struck the chord to which they vibrate, yet at any rate I hope that in some degree I have made them convey to you a definite notion of what we mean by the statement that the symmetry of a crystal is restrained by a law which is no other than the simple fundamental law of crystallography. Furthermore, the simplicity of the point of view we have taken is this, that by it we co-ordinate our knowledge of the molecular structure of the crystal with our knowledge generally of the structure of the universe. Just as the moment Dalton asserted the atomic condition of matter—the moment he invested those little atoms with the character of chemical units, the fundamental law of chemistry sprang into existence: so here the moment that you assume a certain very simple theory for the constitution of a crystal, from that moment the crystallographic law rises up as of necessity, and from it follow also as of necessity those principles of symmetry which I have detailed. Of course I have not gone through anything of the mathematical argument. That does not belong to this sort of lecture, it will suffice us here to know that the results are established. But we can now recognise a beautiful co-ordination of our knowledge of two branches of molecular science, chemistry and crystallography. We have still to understand how it is that matter by this wonderful geometrical instinct plants itself molecule by molecule into this exact architectural structure of a crystal. We have to learn how that is, and there we stand without aid or help to guide us. All that we can assert is something of a confident expectation that we shall find we have to deal with different parts of the same series when, passing from the almost infinitesimal molecules of the crystal world, we come to contemplate the great worlds moving around us, upon their own colossal scale, and that the same laws are operating here as there. The only thing is that we have to take a more general view of the laws to which we attribute the motions of the heavenly bodies. The law of gravitation, for instance, is a very simple law, but we have to remember—because we now know it—that it is but the first term in what

a mathematician calls a series, and for all appreciable distances the first term of the series may be taken as true, but when we get down to smaller distances—when we get to a distance so small, for instance, as that which we are dealing with in the ordinary case of capillary attraction, we leave that term behind and have to consider also other terms in the series; and it is possible when we come down still further to the molecular structure of matter as revealed in the instinctive geometry of the crystal, that we shall have to take yet more terms of the series into account, and more again if we would explain the nature of affinity and combination. But we may at any rate speak with some hope that when we know more of the actual nature of the molecules themselves—whether they be the chemical molecules or an assemblage of such molecules building up a crystallographic molecule—whatever these may prove ultimately to be, we may rest assured that the marvellous instinct which every atom of substance possesses for arranging itself in a crystal, is an instinct which is of the same order as that which bids the celestial bodies move in conic sections when they are under the influence of the law that we are content with calling the law of gravitation.

I will conclude by pointing out to you that this science to which I have drawn attention is one strangely neglected. It is curious that the small amount of mathematics it involves seems to have frightened the usual students of science. The science of crystallography has many vigorous minds working at it in various ways, but it is surprising how very few chemists and how hardly any geologists have in our country studied it; yet the chemist when he wants to show you he has a pure substance tells you of the perfection of its crystallisation. If to night I shall have succeeded in drawing any intelligent person's attention to this wonderful little science—and it is quite within anybody's reach to pursue it—I should feel that the hour I have spent would have been spent admirably well, and the more if I could believe that I had done anything to make the science generally interesting, and in any way more easily studied by those who would turn their attention to it.

THE CHAIRMAN: I am sure you will be glad that in your name I should give our best thanks to Professor Maskelyne for the interesting lecture he has given us this evening and for the information he has afforded us on this very important subject.

ARCTIC DISCOVERY IN CONNECTION WITH THE EXPEDITION NOW MAKING ITS WAY TO THE NORTH POLE.

By Captain Davis, R.N.*

July 29th, 1876.

Mr. Clements Markham, C.B., in the Chair.

The Chairman: It is my pleasing duty to introduce to you Captain Davis. There are few living officers who have had more experience in ice navigation than he has, though in the Antarctic, not the Arctic regions; and this qualification, coupled with his well-known popularity, will, I am sure, ensure to you a very interesting evening.

Captain Davis: Mr. Chairman, Ladies, and Gentlemen,—I must commence with an apology; or, perhaps, considering the locality we are in, a confession would be better. I therefore confess to you that my lecture this evening is not a new one. I had intended to re-write it for this occasion, but when I commenced I found I had only the same material to work with, and I should only have turned my paragraphs end for end; and therefore I have let it remain just as it is. But with regard to

* In consequence of the lamented death of Captain Davis, on the 30th January, 1877, the reports of this lecture and of that on Antarctic Exploration delivered on the 5th August, have not undergone the author's revision.

my lecture to-night, I think there has been a slight mistake. There is not the slightest doubt that Mr Markham and myself should have changed places; I think I should have more amply filled the chair than he does, while he would have given you a more ample lecture. But he happened to be away when I volunteered my services, and they were accepted; and, therefore, I hope that if I can send each one of my audience away with some little grain of knowledge more than they had when they came, I shall have fulfilled my object, whatever your ideas may be.

To attempt to give an outline of Arctic exploration generally, within the limit of time assigned to me, is out of the question. I therefore propose to confine myself to a very brief sketch of those voyages that have been directed to the attempt to reach the Pole itself, prefacing the sketch of those voyages by equally brief remarks on the different routes.

The extent of our knowledge of those vast regions lying between Greenland and Behring Strait, about half a century ago, viz., in 1818, was confined to the sea-coast at the entrance of the Coppermine and Mackenzie Rivers, and the coast-line from Behring Strait to Icy Cape. All the coast between those distant points, and nearly all the land north of it, is due to the discoveries of Englishmen since that date. Surely we may be justly proud of these peaceful honours won by our countrymen.

It is still a moot question as to whom the discovery of the North-West Passage is due, whether to Franklin or M'Clure. I am disposed so to divide the honour as to give the priority of discovery to Sir John Franklin, whilst to Sir Robert M'Clure is undoubtedly due the honour of being the first who ever entered Behring Strait and came out by Davis Strait, although a part of that distance, connecting his discoveries with those of Parry, was not accomplished by ship or boat, but by travelling over the ice.

There are four routes by which an approach to the Pole may be considered feasible.

(1.) By Behring Strait. This route I shall at once dismiss, as being the least feasible of the four, and one which, although often proposed, has never been attempted.

(2.) By the East coast of Greenland. The prospect of success in attaining a high latitude by this route is extremely problematical, the continual drift of ice generally choking the passage between Spitzbergen and the main, and always setting to the southward, renders the chances of working against the stream through the ice small, still it is just possible that lanes of water may be found in favourable seasons within the ice, and even eddies of the main stream setting in the opposite direction, through which a small steamer, by watching opportunities, might work her way north, but, as remarked, success would be extremely problematical.

(3.) The third route is that between Spitzbergen and Novaya Zemlya; but we should more properly describe it now as between Spitzbergen and Franz Joseph Land. Here there is, I believe, far more chance of success than either by Behring Strait or the East coast of Greenland, but all progress in the Polar regions is one of chances; still by observing previous seasons in the matter of the quantity of the moving or travelling ice, even these chances may, in some degree, be calculated, and one season pronounced far more promising than another: thus, the one just past was considered a favourable one, for the quantity of ice that drifted south last year was exceptionally great. In May, June, July, and August its average drift was fully fourteen miles a day. In the two months previous to those it must have been drifting at double that rate. Captain David Gray, an experienced whaling-captain of Peterhead, told me that he considered that nearly the whole of the ice had been driven out of the Arctic basin last summer, and he himself, in latitude 80°, saw no ice, and there was a dark water sky to the northward.

Here I must digress for a moment, just to explain what is meant by a "dark water sky." In the Polar seas we sometimes see a dark purple cloud arising on the horizon, and when that can be seen from the "crow's nest," although ice may be seen so far as the eye can reach, if that sky is seen, beyond there the sea is clear of ice. On the other hand, when you are in quite clear water, as far as the eye can reach, and beyond the horizon, you see a strong white colour—what the navigators call the "ice-

blink,"—that indicates the presence of ice, and a seaman would sooner make his way through the ice towards the purple sky I have mentioned than he would turn the other way where he can see no ice at all.

(4.) The fourth route is that by Baffin Bay, and here it would be possible to start half-a-dozen fresh or branch routes. Each strait between those islands may be considered a route to the Pole; but the particular route of these is that by Smith Sound or between the west coast of the great continent of Greenland and the lands or islands that lie west of it. It cannot be denied that this route has advantages which the others have not, all of which have been considered in determining the path of the present expedition. For instance, there is the encouragement derived from the American expeditions, for although Kane and Hayes did not attain a high latitude, the Polaris did. It is also considered more favourable for sledging; and one axiom in Arctic exploration is in support of it, viz., "Never turn a corner if you can help it;" for you see that Smith Sound is nearly a straight road from Davis Strait and Baffin Bay. This axiom is, however, equally applicable to the Spitzbergen route, but that is essentially a ship route, while that by Smith Sound is considered a sledge route.

There was considerable controversy with regard to the route most advisable for going to the Pole; and very high words arose about it. And when a deputation waited on Mr. Gladstone to suggest an Arctic Expedition,—you all know, I think, that Mr. Gladstone is "up to a thing or two," and so is Mr. Lowe—when the deputation asked for an expedition to go to the North Pole, he turned round, and asked them "which way they wanted to go"? Thereupon there was a split. One wanted to go one way, and another another way; and he said, "I cannot give two Expeditions," and consequently it fell to the ground. Since that, we have grown wiser. I advocated the Spitzbergen route; and so did many others; but we all agreed tacitly that we would hold our tongues—not that we altered our opinion; and therefore we who adventured that route kept silent, and the other party got the expedition by way of Smith's Sound. I only hope we shall be proved wrong by their reaching the Pole before we have had a chance of doing so.

I will now glance at the attempts that have been made to reach the Pole by these three routes, consecutively.

East Greenland Route.—The first voyage made to reach the Pole was by Henry Hudson in 1607. As attempts had been made to reach India and Cathay both by the North-west and North-east Passages without success, it was determined by the Muscovy Company of Merchants to cut the Gordian knot by sailing directly across the Pole ; and Henry Hudson, an experienced seaman and navigator was selected to conduct the expedition, and I beg you will note the means placed at his command, compared with the expedition now on its way for the same purpose. A vessel of 80 tons was allotted to him, and in that, with a crew of ten men and a boy, Hudson left Gravesend on May Day of 1607, to make his way to the North Pole! He made the coast of Greenland in latitude about 69°. Ice lay in with the shore, but he worked his little craft to the northward to latitude 73°, and to the land then seen he gave the name of "Hold with Hope." As he found he could not proceed along the Greenland coast by reason of the ice, he stood across to Spitzbergen to 77° N., where he saw the coast and the ice lying thick upon it. With much difficulty he got round on the north coast and was doubtless within sight of Seven Islands; but after many ineffectual and gallant efforts to get North, and being in want of many necessaries, he was obliged to bear up for England, where he arrived on the 15th September. This expedition, though small, was one of considerable importance, as it made us acquainted with the north and north-west parts of Spitzbergen. Hudson was the first who observed with the dipping needle.

I cannot help digressing again to say that when I read the accounts of these ancient mariners, I fear that their exertions far exceeded those of modern date. When you come to consider for an instant what this man did—how, in that little cockleshell of a boat, he ventured away to the North—and also remember what kind of provisions and clothing and things he must have had —and remember that he started away with ten men and a boy to go to the North Pole,—it tells me forcibly that we cannot do better than our forefathers, and that it is enough to make any intelli-

gent educated seaman of the present day raise his hat from his head at the bare mention of their names.

Keeping to the East Greenland route, we come to the two German expeditions of 1868 and 1869; and although the means by which these gallant Germans sought to reach the Pole were ludicrously small, it is a proof of the spirit which animated the promoters of the undertaking. At the same time it is a proof of their ignorance in sending expeditions totally inadequate for what they were expected to accomplish; thus courting failure, although they deserved better success than attended them. The first expedition, under Captain Koldeway, consisted of a vessel of the same size as its predecessor on that route, 80 tons—the Germania—and the same number of men; and, strange to say, they did much the same as Hudson did, and returned to Bergen without having accomplished much.

Nothing daunted by the non-success of the voyage, great exertions were made to get a second and more efficient expedition afloat, and with success. It consisted of a small steam vessel of 143 tons, strengthened and re-named the Germania whilst the Germania of the previous voyage was re-named the Hansa. Captain Koldeway again commanded, having a brave and active second in Captain Hagemann in the Hansa. They left Bremerhaven on the 15th June, 1869, and made Jan Mayen, then for the edge of the ice off the Greenland coast in latitude $74\frac{3}{4}^{\circ}$. Here they experienced dense fogs, in one of which, owing to a mistake in a signal, they parted company, never to meet again. The Hansa soon got hemmed in by ice, and was lifted seventeen feet by the bow. In such a position the probability of the vessel breaking-up was so imminent that she had to be abandoned for a house built of stones, coal, and snow on the floe, and shortly after this was accomplished the Hansa slipped off and went down. After a winter of incredible suffering, during which they were set down along the Greenland coast, they quitted their icy prison in the boats, and safely rounding Cape Farewell, reached Fredericksthal, from whence they were transferred to their native country. The Germania was more fortunate than her consort. She got to a harbour from whence sledge journeys were made in various direc-

tions. A magnificent fiord was explored in latitude 73°, which was named "Kaiser Franz Joseph." The Germania got clear of the Greenland coast on the 17th August, 1870, and reached Bremerhaven on the 11th of the following month.

Spitzbergen Route.—In 1773 two bomb vessels, the Racehorse and Carcase, selected for their great strength and capacity for stowage, were fitted up to go to the Pole by the Spitzbergen route, under the command of the Hon. Captain Phipps, afterwards Lord Mulgrave. They reached Spitzbergen, proceeding along the west shore, and, after much exertion, reached lat. 80° 36′ N., and returned safely to England. It may be worthy of mention that a lad served in this expedition who was destined to become a great hero, and rise to the highest honour in the service to which he belonged. This was Nelson.

The *Dorothea and Trent*, Captains Buchan and Lieut. Commanding John Franklin left England in May 1818, and reached Spitzbergen on the 7th June. With all their exertions they only reached lat. 80° 34′ and after being nearly wrecked in a heavy gale, in which their ships received such damage as to prevent their attempting to take the ice again, they returned to England.

Parry's Voyage.—After four voyages to the Polar regions, in attempting to make the North-west Passage, Captain Parry was willing to enter on yet another voyage to attempt to reach the Pole by the Spitzbergen route; and as it was considered impracticable to effect it with ships, it was proposed to try to do so by means of sledge boats, to be drawn over the ice, and sailed or rowed through the water, as opportunity offered. The Hecla was the vessel employed on this occasion, and early in 1827 she left England and reached Spitzbergen in June, when they started in two boats, Captain Parry commanding one, and Lieutenant J. Clark Ross the other, taking with them seventy-one days' provisions.

At first the prospect to the northward was favourable, and they experienced no difficulty until they reached lat. 81° 13′, when they were stopped by close ice, and they commenced travelling over it. To avoid the intense glare of the sun from the snow during the day, when the sun's altitude was high, and

which produced snow blindness, they travelled by night and slept by day. Instead of finding the ice smooth, as they expected, it was rough, ragged, and loose, and their work was extremely laborious. On the 30th June they found that they had made only eight miles of northing in five days. On the 12th July they were in lat 82° 14′; the next day 82° 17′; such was the slow progress made owing to the continued set to the southward. On the 20th they found they had only made five miles since the 17th. The men worked cheerfully, as they knew that a reward of £1,000 was to be gained on reaching the 83rd degree, and as they were kept in ignorance of the southerly set, the poor fellows could not well understand why they did not reach the goal. At midnight on the 22nd they reached the highest point that before or since has ever been reached—82° 45′, and then finding they were daily losing ground instead of gaining, they were reluctantly obliged to give it up and return to their ship, reaching her after an absence of 61 days.

There is one man still living who served in that expedition. He was a midshipman in Ross's boat, and is now the present Admiral Bird. Not only is he the only man living who has been so far north, but he has also been the farthest south; so that, in that respect, he stands alone in the world. And I cannot pass over Parry's voyage, without saying a word about Parry himself. A more genial, Christian seaman never existed. I have been much mixed with the Arctic men during my lifetime, and I never heard one word disparaging of the late Sir Edward Parry. He was a genial man in his temper; he knew how to handle men; and he carried them with him wherever he went. I may just tell you one little anecdote, which I heard the late Captain Ross himself tell. It appears when they were away on this Expedition, there was, of course, the usual allowance of tobacco served out every day. I dare say many of you know what "pigtail" means; and it was measured out by inches. Parry always had his measured out the same as the men, though he did not use it. But when they had an extra hard day's work, out came Parry's tobacco, and it was served out for an extra pipe all round. It may appear puerile and ridiculous for me to narrate such an anecdote, but I can tell you that it

had this effect—that these men would have gone to a much hotter place than the North Pole if he had wanted them. On the wall are some blue papers, which are nothing more or less than some of the play bills connected with the Expedition that went out in search of Franklin, and were printed out there. At the bottom of one of them the "Printers Devil" has made a note that the freezing of the ink stopped the printing, and they were obliged to give it up; but they are interesting mementoes of this expedition. The little flag lying on the table is one of those that was used on one of the sledges; and I am happy to say the commander of that sledge, which carried that flag is still living, present in this room, and in good health.

Since Parry's voyage a number have been made in the same direction, but none of them reached within a degree of Parry. The Swedes and Norwegians have done good service here; also our own countrymen, Mr. Lamont and Leigh Smith. In 1871 Lieuts. Payer and Weyprecht, in a small vessel of seventy tons, with a crew of eight men, attempted to reach Gilliesland by following, what is considered to be, the course of the Gulf Stream, hoping to find a passage clear of ice. They reached lat. 78° 41′ N., and found a gradually decreasing depth of water, and from numerous bear tracks on the ice they supposed that land was not far off. Any argument to the effect that the Pole is not to be approached in that direction, from the failure of all these vessels, is not sound; and until such an expedition as is now on its way to the North Pole is turned back in this direction, I will believe it is practicable.

Austrian Expedition.—The fact that Austria had never entered the list of Polar discovery renders the expedition of the Tegethoff remarkable in one sense, and the difference in the mode of proceeding renders it equally remarkable in another; for whilst the navigators of all others did their utmost to take their ships to their discoveries, that of the Austrians took her navigators to theirs. The Tegethoff, a screw steamer of 220 tons, commanded by Capt. Weyprecht, left Bremerhaven on the 13th June, 1872. Weyprecht was accompanied by Lieut. Payer, who had served in the German expedition on the East Greenland coast. The Tegethoff got to

Nova Zembla, and on the 21st August, the ice appearing broken, they tried their luck, but got encompassed with ice the same night, were frozen up, and remained so for two long and cheerless years. The account of this voyage is most interesting. Many a time were they called, in the midst of the long Polar night of 109 days, to save themselves, when by pressure of the ice they thought their ship must founder, and this with a minimum temperature of 51 degrees, or 83 degrees below the freezing point. The ice first bore them to the north-east and then to the north-west. On the last day of August they were surprised by the sudden appearance of mountainous land about fourteen miles to the north. Payer says: "At that moment all our past anxieties were forgotten; impulsively we hastened towards the land, fully aware that we should not be able to get further than the edge of our floe. For months we were doomed to suffer the torments of Tantalus. Close to us, and in fact almost within reach, was a new Polar land, rich with the promise of discoveries, and yet drifting as we were at the mercy of the winds, and surrounded by open fissures, we were unable to get any nearer to it." At the end of October the ship was borne within three miles of one of the islands, and making their way over the hummocky ice, they reached the land in latitude 79° 54′.

The joy of having discovered new land buoyed them up through the second long and dreary winter; and on the 10th March last year, Payer started on their first sledge expedition, but the cold was intense; on one occasion it reached to 58° (90 degrees of frost), and they suffered much. On a second expedition they were enabled to map the country, and on the 26th March they reached latitude 81° 57′, their highest point. A third sledge journey was made, and then, having nailed the colours of their country to the mast of their ship, they abandoned her, and proceeded south with their boats on sledges; and after innumerable difficulties, and with incessant labour, they reached Nova Zembla, from whence a Russian vessel conveyed them to Norway.

Smith Sound Route.—Although Dr. Kane's voyage in the little brig Advance, in 1853, was not one made with the object of reaching the Pole, but was literally one in search of Franklin, it

may be mentioned in connection with one fact bearing on the progress and prospects of the present expedition, viz., the alleged discovery of an open Polar Sea. Dr. Kane himself made no such discovery, but he sent his steward, a man named Morton, who from a cliff in latitude 81° 22′ said he saw an open Polar Sea, with an iceless horizon, and a heavy swell rolling in. Now I can only sum up this important discovery in the very words I did many years ago at the Royal Geographical Society—"Morton was ordered to discover an open Polar Sea, and he obeyed his orders." If he saw water at all, it was but a channel opened by the current in the height of summer.

Dr. Isaac J. Hayes, in a schooner of 133 tons, on reaching Smith Sound, made every effort to get over on the west shore, but pack ice obstructed him. He succeeded in getting into a snug harbour in latitude 78° 17′, where he wintered; and the next spring he made his way by sledge to the west shore. Such were the difficulties he encountered that he was 31 days in getting a distance of 81 miles. He got as far as the 80°, when his men broke down. He, however, pressed on, and finally reached a latitude of 81° 35′, from whence he had the gratification of having the open Polar Sea before him. Dr. Hayes was justly proud of his discovery, and of having reached the most northern land the foot of civilised man had then ever trod. Dr. Hayes succeeded in regaining his vessel after an absence of 61 days; and on the 11th July, 1861, left his harbour and returned to the United States.

Voyage of the Polaris.—Captain Hall, a citizen of the United States, who had spent many years in the Arctic regions in search of Sir John Franklin, and who in 1869 had just returned from a five years' residence in the north, living with the Esquimaux, soon agitated for another expedition; and after much trouble a river gunboat of 387 tons was allotted to him by the Navy Department of the United States, and christened, or rather re-christened, the Polaris. As Hall was no seaman, and was even ignorant of nautical astronomy, a whaling captain—Buddington—was appointed to accompany him; and Dr. Bessels, who had served in one of the German arctic expeditions, accompanied him as naturalist and surgeon.

Hall was to follow in the steps of Kane and Hayes. He sailed in 1871, completing provisions for two and a half years, at Disco; left the most northern Danish settlement in August, pushed up Smith Sound and was most successful. He took the Polaris 250 miles up the strait, and reached a higher latitude than had ever before been attained by any *ship*, and within 30 miles of Parry's farthest; the latitude he attained being 82° 16′. I beg you to notice that I have three times used the word "farthest," but each time with a different meaning. Parry reached the farthest north latitude that ever man reached in a boat; Hall reached the farthest latitude upon the solid earth; and the Polaris reached the highest latitude that ever a ship attained. You see there are three farthests, but all different.

At this extreme latitude Hall's vessel was beset, but a powerful vessel might have forced her way through the ice then seen; moreover a deep water horizon was seen to the north-east, proving that had it not been for the weak steam power of the vessel she might have proceeded much further—so nearly were our Polar honours of getting furthest north, wrested from us. The extreme northern point was reached by the Polaris on the 24th August, and, strange to say, this high latitude was reached without any check or obstacle of any kind.

The winter quarters were in Thank God Bay, in latitude 81° 38′, which the Polaris reached on the 5th September. Three weeks after getting into this harbour, Captain Hall started with a party overland, but he did not get farther than 82°. On his return he was taken ill, became partially paralysed, and died on the 8th November, leaving to others the honours so justly his due.

The year the Polaris reached so high a latitude must have been an unusually mild one, for at Thank God Harbour the ground was free from snow, a creeping herbage covered the ground on which numerous herds of musk oxen found pasture, and rabbits and lemmings abounded. Wild flowers were brilliant, and large flocks of birds passed northwards, while traces of Esquimaux were found.

On the death of Captain Hall the direction of the expedition devolved on Captain Buddington, who seems to have had no

spirit for the enterprise from the beginning, and he resolved to prosecute the voyage no further, but to return as quickly as possible. He did not even form sledge parties in the spring, which might greatly have added to the interest of the voyage. On the 12th August, 1872, the Polaris was again free. When she got as far south as 80° she was beset with ice, and drifted out through Smith Sound into Baffin Bay. In latitude 77½° the ship was so severely nipped, that provisions and boats were got out on the ice ready for deserting her; but suddenly the ice broke up and the ship flew off before a gale of wind, leaving nineteen men, women, and children on the floe, with the boats and provisions. The Polaris was found to have sprung a leak, and the water was rising in the hold to such an extent that Captain Buddington ran his ship on shore near Lyttleton Island. Here they wintered, and having built boats from the timber and planking of the ship, they all embarked on 3rd June, 1873, and were picked up by an English whaler off Cape York, The unfortunates who were left on the floe drifted down Baffin Bay, and were eventually picked up in latitude 53½°, near Wolf Island.

Of the crew that accompanied poor Hall only about nine were native-born Americans. The expedition seems to have been badly organised, and there was a want of discipline and order in the whole conduct of the voyage, which proves that those who embark in such undertakings require to be under good law and discipline. The great success that attended the Polaris voyage was not attributable in the least to order; but the non-following up of that success may be fully attributed to a want of it. Permit me to pay a tribute to Dr. Bessels, a German, who showed himself throughout the voyage to be a man of pluck and spirit and a truly scientific observer. I fear if the whole crew of the Polaris had been animated by the same spirit as Dr. Bessels, we should have no need of sending our own expedition to reach the North Pole.

THE PRESENT ARCTIC EXPEDITION.

As is now well known, the two ships selected for Arctic service were the *Alert*, one of Her Majesty's ships, of between seven and

eight hundred tons, and the *Discovery*, formerly the Bloodhound, a whaler of nearly six hundred tons, both vessels having steam power.

Captain George S. Nares, who recently commanded the Challenger in her deep-sea exploring expedition, commands the expedition in the larger vessel, and Captain Stephenson is second in command.

I may digress for one moment again, to say a word with regard to Captain Nares. I have a slight knowledge of him, and I am very pleased to say, as far as my knowledge goes, he is the right man in the right place. There is so much doubt sometimes in the public mind whether interest, or some fluke or other, as connected with government, gets a man a situation, that I am pleased to say, with regard to Captain Nares, I do not think there is any other man whom they could have selected. He was out in the *Challenger*, and although I do not say he is a man of science, still he is a scientific man, and he has good social qualities, which is also of great consequence.

A large number of astronomical and other instruments are on board, and every device that human ingenuity can suggest to help them in their great work has been executed, and nothing that forethought and money can procure is wanting. In point of fact, on these two ships, their fittings and equipments of all kinds, the information derived from a century's experience of Arctic navigation has been brought to bear, and it is not mere assertion to state, that never did an expedition leave our shores more replete in every particular for the service intended than the Alert and Discovery. To describe the mode of strengthening the ships to withstand the enormous pressure of the ice would be too technical for general understanding. They are fitted with water-tight compartments, so that if the ship is stove in in one part she is in no danger of sinking. It is to be hoped that in case of necessity they will prove more useful than those of the Vanguard did; but it must be remembered that the Vanguard was a heavily armoured iron ship, and the Alert and Discovery are wooden ships.

To show you the necessity for great strength in these ships, I may mention that in the *Terror*, the beams along the deck were

22 inches square, and yet by the sheer force of the ice on the sides of the vessel these beams were forced up. A short time ago I was speaking to Sir George Back about it; and he said, "I perfectly remember it myself. I was reading prayers to the company, and I felt myself lifted by the feet." You all know the force required even on a match, to break it end on; and you may imagine what strength is required in a ship to withstand such a pressure as that. I may say that the first week it was known that an expedition would go, letters came into the Admiralty from 238 officers wishing to go; and I believe myself, if all the volunteers had been taken, you would not have had one left at home to take care of the coast. The men were not taken hap-hazard, but every man was selected not merely for his physical strength—though all, both officers and men, had to undergo a severe medical examination; but they were examined for their social qualities. I was told, though I cannot vouch for the truth of it, that one was asked the question—"What can you do to amuse the ship's company!" He said he did not know. They asked him, "Can you play the fiddle?" No; he could not play the fiddle. "Can you play the Jew's harp?" No; he could not play that; he could not play anything. "Well, what can you do?" "Well," he said, "I can play the fool a little now and then;" and so he was shipped at once; and I hope he has been playing the fool occasionally to amuse the ship's company.

The screws are fitted in a way to admit of their being readily detached and raised out of the way of damage from the ice, while the screw shafts can be drawn in.

Both vessels carry an unusual number of boats, all being constructed in a peculiar manner to meet the contingencies to which they will be exposed. They are also furnished with collapsable boats.

Next in importance to the ships and the boats, as a means of effecting the objects of the expedition, are the sledges, and here, fortunately, the expedition has the advantage of the experience of Sir Leopold M'Clintock, who may be considered to have brought sledge travelling to perfection in the numerous Arctic expeditions to which he has been attached.

The sledges are made of American elm, a tough light wood, the principle of construction being: (1) Two steel-shod runners, curving upwards at the ends, these ends being united by longitudinal bars, supported by columns resting on the runners; and, (2) Cross-bars on the top, keeping the runners apart and parallel. These cross-bars, with a piece of canvas stretched over them as a kind of sacking, is the platform on which the tent, provisions, and all the *impedimenta* of travelling are carried.

No screws or nails are used in the construction, excepting those securing the steel runners to the wood—all the other parts being united with hide, strips of which, soaked in hot water, are used for lashing the various parts together. In drying, the hide contracts and forms a stronger security than nails or screws could. Nails and screws would break by the extreme cold.

Of these sledges no fewer than between thirty and forty are supplied to the ships, varying in size from that adapted for twelve men to those suited to five; and also dog-sledges.

In dragging the sledges, the men wear a canvas belt over one shoulder and under the other arm, according to which side they are on. At the end of the belt is a button which, by a single turn of the lanyard round the tow-rope, at the turk's-head worked on it, remains quite secure as long as there is a strain on it, but the moment it is slackened it disengages. This keeps the men up to the mark in pulling their quota, and prevents accidents in case of the sledge breaking through the ice.

The starting load of an eight-man sledge is calculated at 1646 lb., or about three quarters of a ton; this gives an average of about 2 cwt. to each man, which weight is considered the maximum for trained men.

Next in importance to the sledges are the tents. These are of three different sizes, to accommodate twelve, eight, or five, men. They are made of unbleached duck. The eight-man tent is nine and a half feet long, seven feet wide, and the same high; weight 30 lb.; the others proportionately larger or smaller. The ends of the tents are spread by two poles crossed at the top, each pole end fitting into a canvas cap fitted to the tent for the purpose. The tent is secured in an upright position by a ridge-rope and

stays, the latter at one end secured to the sledge placed transversely to the tent, and the other to some of the heavy gear, or to a lump of ice ; two spans, set up to pegs, keep the sides distended, and two half hoops at the top help to make the tent more roomy by spreading the upper angle. A foot-cloth about a foot wide is attached to the lower edge of the tent, on which the snow is shovelled to keep the tent steady, and also to keep the wind out. Three or four small tubes of canvas serve the purpose of ventilators.

There is a flap or window at the inner end of the tent, that can be opened or shut at pleasure from within. The tent furniture consists of a macintosh sheet, which is spread over the ground. This is covered with a duck floor-cloth, on which the duffle blanket is placed, which serves as a bed for the whole party. Then each man has his duffle sleeping-bag, with his knapsack for a pillow; a large double duffle counterpane covers the whole family. The sleeping bag is so arranged that a man can either sit up in it and take his meals, or almost hermetically close himself within it. In very cold weather a blanket foot-bag is added to his other luxuries; but when the cold becomes very extreme, especially when accompanied with wind, a tent becomes untenable, and recourse is had to building a snow hut, after the manner of the Esquimaux.

The cooking apparatus is circular, made of tin, with wooden covers, the heating material being spirits of stearine used with a cotton wick. By an arrangement of the saucepans two or three articles can be cooked at the same time, the whole being protected from the weather by an outside coat made of fear nought.

Everything is arranged for sledge travelling in a most methodical manner, and although each man's appetite cannot well be measured, that which is intended to stay—if not to satisfy it—is, and that to a great nicety. Each man's wardrobe is also arranged for him, beyond which he dare not take the weight of an ounce. All is stowed on the sledge in such a way that each article can be readily found at a moment's notice. The tent-poles, pickaxe, shovel, etc., are kept outside, as is also the cooking stove on the netting at the extremity of the sledge

'Whilst on the subject of the travelling equipages, it may not be out of place to speak of the travelling itself, and this differs much with the season both in point of speed and comfort in travelling, for as in spring time the road is good, the extremely low temperature at that period of the year makes it uncomfortable, and each man has to keep a sharp look out for his neighbour's nose, as one is quite unconscious himself of the terrible frost-bite, although it is easily detected by another; and at times you are literally dependent on your fellow-worker for the safety of your organ of smelling. The dress at this time is of the warmest of woollen garments, not furs, beyond the seal-skin cap, and the whole covered with a suit of duck, as being most impervious to the blinding, penetrating snow dust, which, if it gets into the cloth, soon makes it like boards.

The travelling in summer, say July, when from the heat of the midsummer sun the thaw has set in, is perhaps, more uncomfortable than spring travelling; for although the men have doffed the very heavy clothing and taken to the more civilised suit of the temperate zone, they have to wade through sludge and just melted ice without certainty of footing, and often stumbling and slipping, and with no prospect of drying their clothes when the day's work is done; so that of the two, the more severe cold of April travelling is preferable.

We will take an ordinary day's travelling without the extremes, for an example, the "hurrahs" and "God speeds" of parting from our shipmates being the memory of a week, with the men well settled into their harness and daily work. After an early breakfast of chocolate and a little meat and biscuit, everything is packed for the day's journey; the men take their places at the drag-rope and away they go, considerably refreshed in one way from their night's rest, but so cold, that their fingers—notwithstanding the warm mitts—may be said to be "all thumbs." But the exercise of an hour's hard work restores circulation, and I suppose the next three or four hours may be considered the most comfortable period of the twenty-four, as the cravings of hunger have not set in, and the feeling of fatigue has not come over them, so they trudge along cheerily, and even merrily. The

officer generally forms the advance guard, not only to give the line of direction, but also to select the smoothest road for the sledge—a strict attention to these duties greatly assisting the labour of the men. Occasionally he drops alongside his men for companionship, with a kind word to one and a joke to another, and stimulating exertion in all, to attain some object or some goal he has fixed on to reach that evening, or even giving a willing relief at the drag-rope to some poor fellow who is more than ordinarily done up; and so the day goes on. The period and length of the midday halt depends generally on circumstances and the necessities of the men themselves; they then lay into the rope again, and the last half of the day becomes more laborious than the first. Then comes the happy time—the halt for the night. A spot is selected for the stoppage—not on the earth, even if possible, for that is frozen, and gives off no warmth; the ice or snow is preferable. Then unpacking commences, every man having his special duty to attend to. The cook for the day, and his mate, care not for the tent; the other men attend to that. The tent is pitched in a few minutes, the position regulated by the direction of the wind, and the door being always on the lee side. The sledge is placed in position, the tent-man brushes out the floor of the tent, then spreads the macintosh, unbends and kneads out the duffle bedcloth as well as he can, but no amount of kneading will make it flat; the stiff ridges and corners must await an hour of the heating from the bodies of the occupants before it will assume anything like the flat surface intended. The sleeping bags are arranged for the night, that for the officer at the head, or inner end of the tent, and that for the cook at the foot, or near the door, so as to be ready for preparing the early morning meal. The officer probably takes observations, and then all—with one exception—take to the sleeping bags, and all are cold and sleepy; the pipe, which is a real luxury in that cold climate, keeping a few awake. The individual excepted is decidedly the most important man of the evening—the cook. Directly the sledge stops he arranges and trims his lamp to thaw the snow, whilst another man takes the frozen pemmican and chops it up with the pemmican axe into

small pieces, the splinters flying off as if he were chopping a lump of rock. By the time he has chopped up the allowance for the meal, the snow in the kettle has thawed, and the meat is put in, the cook carefully tending it until it is boiling. Then, at the sound of the dinner bell—which consists of a string of pannikins being rattled together—all rise up and a pint of good stout strong nodge-podge, smoking hot, is handed into each blanket, which opens and falls back like the head of a barouche, whilst the occupant takes his dinner; and, as the warmth of the hot condiment acts on the system, so do the spirits of the party revive, and the cheerful yarn or even the song goes round. But the poor cook has yet to make tea, the pannikins being scraped out as well as possible to hold the amber fluid. At last, tea over, the cook arranges his cooking utensils for the early breakfast, and then creeps shivering into his bag; the pipes are lighted, and what with the smoke and the animal heat of eight bodies, the blankets begin to thaw, the duffle bedcloth to lie flat, and soon all are asleep.

Thus we have given an idea of a favourable day's march; but when we came to snowstorms and the blinding driving snowdrift—when we came to be brought up several times in the day, and as often the sledge has to be unloaded and reloaded—or the sledge falling through the ice and everything getting soaked—or any of the thousand-and-one accidents of sledge travelling—who can paint the utter misery of a day's travelling? When the men, or some of them, are ready to lie down and give up dear life itself, if only allowed; and it requires all the reasoning, nay, firm authority, of the officer, to keep his men together.

Great assistance can be rendered by the use of dogs, and the advantages of a good pack are very great, especially when short-handed, as two dogs are considered equal to one man in sledge-hauling; and instead of consuming one man's food they require but a small allowance, and more, require no clothes to wear or carry. M'Clintock sent six or seven dogs with six men, and this party worked harmoniously together over a thousand miles and through the lengthened period of nearly eighty days.

Dog-driving is no easy matter, and many here who may drive tandem or handle the reins of a four-in-hand with consummate ability would find themselves all adrift in the attempt to drive a team of dogs. The harness consists of a few strips of canvas and a single trace about twelve feet long. The dogs are harnessed three abreast, with a leader. The first difficulty is to catch your dogs and harness them—a not very easy job, as every dog strives to do exactly what you do not wish him to do; but at last they are all harnessed and a start is made, and away they go, the driver guiding them entirely by the whip, which it is necessary for him to handle effectually with either hand, and it has to be kept continually at work. As the dogs at the sides are most exposed to the whip, they naturally try to become the middle dogs by jumping over the backs of their neighbours; so that after a short time the traces get so plaited together that the dogs cannot work and a halt becomes a necessity, and at the risk of frozen fingers the driver has to divest himself of his mitts to disentangle the traces and get the team into order again.

It is not an unusual thing that when a dog feels the lash, he immediately bites his neighbour, who bites the next dog, and so on, until there is a general fight and howling. The lash is then no longer of any use, and the driver is compelled to restore order with the handle of his whip.

The moment the whip ceases, and halt for the night takes place, the dogs fall asleep and remain motionless; but at the first sound of the pemmican axe in chopping up food, they start up like so many famished wolves, and surround the chopper, darting upon any splinters of meat which fly off. The dogs are not fed until an hour after halting; then their food—usually frozen bear or seal's flesh—is strewed over the snow and trampled into it, so that when the rush takes place the weak dogs are enabled to get their fair share of the food with the strong. Everything that is eatable or gnawable is carefully kept out of the reach of the dogs; and woe be to the unlucky wight who leaves a pair of boots within their reach.

Some doubts were entertained whether dogs would be procurable for the present expedition, owing to a disease that has been

prevalent amongst them, by which many have been carried off; and an Esquimaux is loth to part with his team, as by his team he is estimated, and an Esquimaux without dogs becomes a mere hanger-on to one who has; but I am glad to say that Captain Nares was enabled to obtain twenty dogs at Disco, twenty more at Ritenbenk, and the remainder of the sixty he required he expected to obtain at Upernavik or Proven. Moreover, he has obtained the services of two Esquimaux as dog-masters.

These dogs are most extraordinary animals, and I might tell you one anecdote, which I heard from Captain Young, of the Pandora. In all these teams of dogs there is one that becomes naturally the bully of the rest—the "top-sawyer," and he takes command over all the rest. On one occasion, they lost this top-sawyer, or bully dog, and he was away for some days. Then he came, as Captain Young said, "nothing but skin and bone." It appeared they put some food down to him; but before he touched the food, he went round to every dog, curled up his back, and snarled at them, as much as to say, "I am still nasty."

It is impossible to tell how far the expedition will go, or what will befall our explorers; but we now know by the return of the Pandora that the ships had safely crossed the much dreaded Melville Bay—that *bête noire* of the whalers, for it was said of old, and with some truth, that if caught between the fast ice and the moving pack, the ice would either pass under you, over you, or through you.

But steam is a great assistance in taking a ship through, and it has helped Captain Nares on to the entrance of Smith Sound, where, at Carey Islands, Captain Nares landed and left his first despatches, by burying them enclosed in a tin case, and then erecting a cairn at a certain distance and bearing from the buried despatches, according to an understood arrangement. These despatches were found and brought home by Captain Allen Young. Any vessel sent after the expedition will at once know where to search for despatch cases, which will be buried on different headlands as the expedition proceeds. Every endeavour will be made to get the ships over on the west side of the Sound, and then they will work to get to the northward.

I cannot be so bold as some in at once taking the expedition as far as the Polaris went, for, from the evidence of Kane and Hayes, the year the Polaris got so far north must have been exceptional; but I think I may in imagination take them midway between the position the Polaris obtained, and that at which Hayes and Kane were stopped, say latitude 81° or 81½°. But it is not intended that both the ships shall go to the extreme point possible. It is arranged that one shall be left in a position where there will be little danger of not being extricated, whilst the other will be the advance ship, and go as far as possible. The advantage of this arrangement is evident; for if the advance ship is beset beyond the power of being freed, the officers and crew will have the other ship to fall back on. We will place the rear ship in latitude 80°.

I would wish to be guarded in what I state in regard to the progress of the ships up Smith Sound, as there are good hydrographical reasons for believing that the ice is more open, and the strait consequently more free than is generally supposed. For instance, a piece of wood about a foot long was picked up in latitude 82°, and an Esquimaux stated that plenty of wood came from the northward, and is washed up along the shore of Grinnel Land, borne by the current from the coast of Siberia. Then again, a distinct tidal wave is said to meet at Cape Fraser, on the west coast of Grinnel Land; that is, to the southward of Cape Fraser the flood tide makes to the northward; and to the northward of the Cape it flows south; and at spring tides, or the full and change of the moon, there is a rise and fall of 5½ feet.

These are most important considerations; still, the ice and the narrow straits are the great difficulties; and I believe I shall be found right in limiting, as I have, the position attained.

In speaking of the different routes, I have stated that the one this expedition is pursuing is considered to be essentially a sledge route; and it is the opinion of those connected with it, that sledging is before them to a great extent, as indeed the number and variety of the sledges taken with them, indicate. Now the first thing to be done when the ships are in the positions I have assigned to them (or wherever they may be) will be to open a

communication with each other. The advance ship will know the position of the rear ship, and the rear ship will know somewhat of the direction taken by her consort; and it is not too much to say that that communication will be effected before the winter sets in.

We have now the advance ship 90 or 100 miles from the rear ship, and eight and a half or nine degrees—or say 500 miles—from the Pole. Depôts of provisions will probably be established between the vessels, the surrounding country explored in short autumn trips with the dogs, and then they will make themselves comfortable for the winter, and speculate on reaching the Pole next summer.

With the appearance of the sun the work will commence. Some of the officers and men of the Discovery, the rear ship, will be transferred to the Alert, the advance ship, to aid in the great object of the expedition. The sledging parties will be organised, and an advance made towards the Pole.

It is very difficult to speculate on the progress they will make. M'Clintock, on his excursions, averaged a distance of thirteen or fourteen miles a day, whilst Hayes, in Kennedy Channel, averaged only two or three miles; in the one case the ice was tolerably smooth round the shores of the islands which M'Clintock traversed, whilst the ice in Kennedy Channel was hummocky. Then, again, the distance from the Pole, 500 miles, is as the crow flies, or in a straight line, and as we know that the coast does not continue long to follow a straight line, and to go "across country" is not possible, we must therefore, in estimating the distance from the Pole, take that into account, either by adding to the distance or deducting it from the daily average distance of travelling, and I do not think I shall be wrong in estimating the daily average at seven or eight miles at the utmost, or by nearly doubling the actual distance. Taking then, the average daily advance at eight miles, it would take 63 days to reach the Pole from the advance ship, and that time doubled for the return would make 126 days' travelling. Now as no one sledge could do this, it must be effected on the telescopic principle, and also on Darwin's principle of selection of species. Depôts or stations will be formed on the

route, to which sledges from the ship will convey provisions and return for more, or meet and replenish returning sledges, whilst the selection will go on with regard to the men and the dogs in order to secure the "survival of the fittest" for the last draw, or the small end of the telescope, or that party which will push forward from the most advanced depôt to reach the Pole, whilst the second slide will prepare to go to the aid of the first on its return, and so on back to the ship.

It is a very easy matter on paper to cut and dry all that can or should be done, but it is a very different matter to those concerned. You cannot send a ship to the North Pole by Act of Parliament; and a very trifling matter may set aside the most elaborate calculations. Like Cæsar's "*Veni, vidi, vici*," the instructions might be resolved into almost as few words—To the North Pole—by Smith Sound—go ahead!

Never has an expedition created such universal interest, and never have the papers been more full of articles, comments, descriptions, and illustrations, than they have been in regard to this Arctic expedition. Never have officers and men been so *fêted* and made much of; never has the general public been inspired with such enthusiasm for a Polar expedition. Never have the articles of a lady's *trousseau* been inspected with more interest than the articles of clothing which the officers and men are now wearing. Their tents and bedding were minutely examined by the fairest of the fair—enough to turn the brains of the unsophisticated fellows who were to sleep in them. The cooking utensils, pannikins, and spoons, were objects of interest; the duffle cloth was handled and felt; and I am only surprised that duffle worked into *paniers* and *tabliers* are not the prevailing fashion this winter, and the true Arctic cut, something to be particular in.

But it is no light task that Captain Nares and his companions have before them; and although I most strongly deprecate creating a visionary belief that the Pole will be attained, as most unfair to our gallant countrymen, I may confidently assert that every effort will be made that human beings can make. Many circumstances combine to increase the difficulties our countrymen will have to contend with. Let us remember that Captain Nares him-

self is the only officer in the two ships experienced in ice work, and it requires some apprenticeship to get your hand in that kind of work. Let us remember also that the ships, small as they looked amongst the ironclad monsters in the docks at Portsmouth, are nevertheless twice the size of any of the ships that have preceded them in Arctic exploration; and a ship of seven or eight hundred tons is no joke to box about amongst the ice, or get off the ground if she tails on, and that with no more hands than were accorded to the smaller ships.

Then, again, on what grounds is it reasoned that the distance between the supposed position attained by the advance ship and the Pole is adapted for sledging? I see more reason to argue to the contrary, from the existence of those islands, separated by wide channels that lie west of the great continent of Greenland, and which may continue to the Pole; so that to take our voyagers to the Pole and bring them home the year after next, is to lead the public to expect too much, and as a matter of course they would be proportionately disappointed if it be not accomplished. It is at the best a matter of chance, which, if unfavourable, the boldest and bravest may fail to achieve success; but if circumstances are favourable we may be sure that advantage will be taken of them to the utmost. The explorers will do their duty equally in both cases.

From the perusal of the journals and narratives of previous Arctic voyagers, from the lips of many of them I have been associated with, and from a little personal experience, *I* know full well what Captain Nares and his companions will have to go through and suffer and endure under any circumstances. *They* know how much every man, woman, and child in England who understands anything about it wishes them "God speed," and the last telegram they received before leaving Portsmouth was from Her Majesty to that effect. *We* know that the men who have gone to attempt to reach the Pole are British sailors; and although one myself, and it may seem presumption, assumption, or conceit on my part to say it; but *if* it is to be done, cheered on as they will be by the thought of "What will they say in England?" *they will do it*, for the honour of Old England—and, to the glory of God.

The CHAIRMAN: I have only to propose a very hearty vote of thanks to Captain Davis for his most interesting lecture, on a subject which is attracting such great attention among us, now that our gallant countrymen have just completed the most persistent attempt ever made to explore these unknown regions. They have probably just returned to their ships—very likely only three days ago. I beg to return our most sincere thanks to Captain Davis for his most interesting lecture.

ELECTRICITY AS A MOTIVE POWER.

By Professor G. Carey Foster, F.R.S.

July 31st.

Dr. Siemens, F.R.S., in the Chair.

The Chairman: Ladies and Gentlemen,—Having been requested to take the chair this evening, I have the pleasure of introducing Professor Carey Foster, the lecturer for the evening.

Professor Carey Foster: The subject of this evening's lecture is, as you are probably aware, Electricity as a Motive Power. There are almost innumerable methods by which motion in a visible mass can be produced by electrical agency. The most usual methods of ascertaining whether a body is electrified or not are methods by which it is made to give motion to a visible mass of matter. An ordinary electroscope is an instrument in which an electrified body is employed to give motion, it may be to gold leaves, or pith balls, or some easily movable bodies. Again, if we want,—not to detect the electrification of a body,—but to ascertain that an electric current is passing along a wire, the most usual instrument to employ for the purpose is a galvanometer, an instrument in which a magnetic needle is moved from its ordinary position to a different one. I may say, then, that the usual way of detecting electrification is by causing motion in some light bodies, and the usual way of detecting an electric current is by causing motion in a magnetic needle.

I shall not take up time by showing these precise experiments; but of the innumerable other illustrations of the production of motion by the medium of electricity, which I might have selected from the magnificent collection in this building, I will attempt merely one as an illustration of the ways in which motion can be produced by means of what is commonly called statical electricity. At this end of the table you will notice what I may call a little circular railway. There is a glass plate with a circle of tinfoil pasted upon it and a circular brass rim rising a little way above it. Then there is a glass ball which is capable of rolling so as to remain in contact with each of these. If by means of an electrical machine I electrify the brass rim and connect the tinfoil with the earth, then this little ball, if I give it a small impulse, will continue to move round and round. No doubt you are all aware that experiments of this kind do not succeed well in a damp atmosphere; it is difficult under such circumstances, to keep electricity when we have got it, and it is possible I may fail this evening on account of the very damp state of the weather, but I will make the attempt.

I will in a few words state the general nature of the action. The portion of the ball which at any instant rests against the brass rim becomes electrified by contact with the rim. The rim is electrified by the machine, the glass becomes electrified in the same way and is repelled, so that the ball rolls away, and a fresh part of the glass comes in contact with the rim; then this part becomes electrified, it rolls away, and so the motion is continued.

Between the experiment you have just seen and the deflection of the gold leaves of an ordinary electroscope, there is one important difference. We have here a continuous movement: we can keep the ball rolling as long as we keep the machine at work; but when we deflect the electroscope we get a movement to begin with, and then the whole thing is at rest. By the use, not of statical charges of electricity, but of electric currents, there are many ways by which we can produce continuous movements. One of those I hope to be able to show you by means of this apparatus. It is a magnet set vertically and capable of rotating very freely about its own axis. If we send an electric current from the bottom to the

middle of the magnet, then, if the current is strong enough and the magnet is sufficiently free to move, it will spin one way or the other according to the direction of the current. We have a current passing along the magnet to the middle, and the magnet rotating about the current which passes through itself; or, if you like to state it differently, it is just as correct to say either that the magnet is turning about the current, or that the current in the magnet is rotating about the axis of the magnet itself. It is a mutual rotation of a current and a magnet about each other. As soon as I cause the current to pass, the magnet begins to spin; if I stop the current, the motion will cease very quickly on account of the friction; and if I make the current go in the opposite direction, we shall get rotation in the opposite direction. In this little apparatus I shall be able to show you, I hope, another form of what is essentially the same phenomenon. There is here a fixed magnet — this little bit of steel at the bottom, — and there is a conducting wire hung by a loop at the top and dipping into the mercury in this little wooden trough. This piece of steel, as I have said, is a magnet. If I allow the current to pass down the wire into the mercury, or to pass up the wire from the mercury, in either case, if the current is strong enough, the wire will rotate round the pole of the magnet. I may mention in passing that this rotation, the movement of a wire under these circumstances, was one of the first electrical discoveries made by Faraday. When the current passes, you see the wire moving, turning round and round under the influence of the magnet, and by inverting the direction of the current we can invert the motion of the wire.

In all these cases however, although we have got motion, the force that is brought into play is very small. The apparatus that we employ must be very freely movable in order to enable us to get a visible motion produced at all; and although the movement can be got, we cannot say that we get it in a practical way. We cannot get sufficient force exerted by these methods to be useful for any practical purpose If we can get the apparatus themselves to turn, that is as much as we can do; we could not employ any of them to turn anything else. But by taking advantage

of another property of electrical currents, we can produce very great degrees of force indeed. I will just point out the chief features of an apparatus I am going to use to illustrate this. There is first of all, a piece of soft iron bent into an approximately horse-shoe form. (In speaking of electrical and magnetic apparatus, we are accustomed to use the term "horse-shoe" in rather a liberal way, to embrace a form of thing which is not at all like a horse-shoe, but more like a U.) A copper wire is coiled round this, and when an electric current passes through the wire, the iron which at present is just like common iron becomes magnetic. If I put another piece of iron to it, at present there is no perceptible action of one upon the other; but when a current passes through this coil of wire, the iron inside the coil becomes an exceedingly strong magnet. To show the magnetic power we will put on some weights. I am not attempting to bring out all the force of the magnet, and a larger battery than we are employing would be required for the purpose. I have here only five cells, which is a very small battery, but still that produces very considerable magnetic force, so that we have a force which really might be employed to produce results on a practical scale. In this experiment all that I have shown you is that a piece of iron is held with very great force against the poles of the magnet; but I might have made the experiment in a slightly different manner. Instead of letting the iron be in contact with the poles to begin with, I might have held it a little away from them, and then it would have been forcibly drawn up against them, so that we could have got motion produced through a small distance, but with very great force. Now a circumstance which renders this effect of electric currents still more available for producing effects of mechanical motion than even what I have shown you would imply, is that the same current can be employed to magnetise any number of pieces of iron. There is some more iron in this second apparatus which can be magnetised in the same way. There are two iron cylinders connected by a cross-piece at the bottom, and there are coils round them through which the electric current can be passed. When a current is circulating in the coils, the iron becomes magnetised, and the ends of the iron will hold a piece of soft iron with such great force that I shall not be

able to take it away. I wish to show you by this that the same current can be made to magnetise this piece and this, or we might have a half-a-dozen more which would be all magnetised in the same way. Now I have stopped the current and the magnetic force has ceased. A still further fact connected with the production of magnetic force by an electric current is that we have the power of producing and destroying the magnetism at will. As long as the current is passing we have here a strong magnet, but when the current ceases the magnetism ceases to exist. Now suppose this piece of iron which I am holding in my hand to be fixed upon a vertical axle, so that it could turn in this way, and that while it is in this position a current begins to pass through this coil. Then the iron comes into contact with the poles. Now let the current cease as soon as the iron is in contact with the poles; then by the momentum it has acquired in obeying the current and moving towards the poles, it will be carried beyond them. Suppose that the force ceases to act just at the instant when the iron is opposite the poles, then the momentum it has got will carry it beyond. Now imagine the current to begin again, and the iron will be turned round into that position; let it cease again, it will be again moved forward, so that by making and breaking the electric circuit so as to allow the current to pass and then to interrupt it again at the proper instant, we could get a continuous rotation in a piece of iron. And it is quite easy to construct a mechanism so that the motion of the iron shall itself make and break the passage of the current, so as in fact to make the apparatus self-acting. That is very roughly and in general terms the principle of the arrangements which have been found the most effective for producing movements on a large scale by means of electricity in any form.

To carry out the kind of action I have been referring to, very numerous contrivances have been devised. I have here upon the middle of the table a series of three instruments, not exactly such as would be obtained by carrying out the arrangement I spoke of, but still essentially on the same principle. These were devised by Sir Charles Wheatstone, and are contributed to the Exhibition by the Council of King's College, being part of the collection of Sir Charles Wheatstone's apparatus.

which he bequeathed to the college. I am sorry to say that they are a little antiquated, and are not quite in working order. Very little indeed in the way of repair would probably make them work, but they are not in such a condition that I am able to show you them to-night in actual motion. The principle of them all is the utilization of the magnetic property imparted to iron when an electric current passes round it. You may notice in this instrument a piece of iron at the bottom, and there is another at the side nearest to me, each of which is surrounded by a coil of wire; when the electric current passes through the coils, each piece is rendered magnetic so that it attracts any piece of iron in its neighbourhood. You notice also that on the circumference of this wheel there are four pieces of iron. The circumference of the wheel is cut, as I may say, into steps, so that one end of each bit of iron is farther from the magnet than the other end; at the bottom of the step it is farther from the magnet than it is at the top, so that if you have this iron magnetised it attracts the piece of iron on the wheel, the iron virtually gets nearer to it as the wheel turns, but as soon as the iron has got as near as it can, by a self-acting arrangement the magnetism ceases, so that the wheel is allowed to go on; and another piece of iron is acted upon in exactly the same way, and so on in succession. In the next apparatus we have the same sort of action. There is a disc of iron which alternately approaches and recedes from four magnets placed around the apparatus, and in doing so it gives motion to the rest of the machine. The electric current is allowed to pass alternately around the four magnets in succession, pulling in the wheel first towards one and then towards another, and the way in which the wheel is mounted causes that kind of motion to produce rotatory motion in the fly wheel. The action of the third apparatus is very similar.

As I have said, none of these are in sufficiently perfect condition to be usable, but there are instruments here by which I shall be able to get movement produced. In this one, we have what are called electro-magnets, that is, pieces of iron which become magnets when an electric current passes through the wire coiled round the iron. In this instrument there are four

such magnets, and the current from a battery can be made to circulate in the coil of each of these in succession. Suppose that this magnet is excited by the current, then it will attract a piece of soft iron carried by this wheel. If these pieces are near the poles of the magnet, they will be attracted till they come opposite to them, but as soon as they get as near as they can, by a self-acting arrangement, the current is cut off from the coil so that the iron ceases to be a magnet. As soon as the iron gets near the magnet the force which attracted it ceases, so that it is free to go away again without hindrance; but as soon as the first magnet ceases to act, another one of the set begins to act and so it continues the movement. The action is taken up by the four magnets successively round the circle. Still more might be put in, but these four act quite well. As soon as I allow the current to pass we get a very active movement, and by way of showing how such an engine might be utilised, we have it working this little apparatus, so that we are actually pumping water by means of the electric current.

Here is another apparatus, which depends essentially on the same principles again, but carried out in a somewhat different manner, which I will show you the action of. Here again there is a very perceptible amount of force exerted; it requires a perceptible exercise of force to prevent the machine turning. It will be difficult to make the action of this apparatus perfectly intelligible to those who have no acquaintance whatever with electrical apparatus, but to those who have (and no doubt many present are more or less acquainted with such instruments), I may mention that the movement you have just seen is essentially similar to that of the galvanometer needle. This frame is coiled with wire, just like the coil of a galvanometer, and this revolving piece is a magnet when a current is passing through the apparatus, and if we begin with it in this position it will be deflected through a quarter of a circle, one way or the other, according to the direction in which the current passes. Then suppose the current to be inverted when the revolving piece has got here: the direction of the force between the fixed coil and the revolving piece will be inverted also, and the latter will turn half-round; if then the current is inverted again, the revolving piece will be urged into the same

position as at first, and so we shall get continuous motion. The current must pass in one direction as the machine moves from this position to that, and then in the opposite direction as it moves from that position to this. The current must be inverted in direction at every half rotation. If you think of the action of a galvanometer needle, and can imagine that the magnetism of the needle is inverted at every half rotation, but the current always goes the same way through the coil, you will easily see that the needle will maintain a continuous rotation. That is virtually what is done in this machine.

Then we have still another machine on the table which I see. I shall not be able to show you the action of, as the mercury at the bottom has got displaced. It is one in which a series of four fixed electro-magnets act on four movable electro-magnets, attracting them as they are approaching and repelling them as they recede, in that way producing continuous movement. But these experiments are sufficient to show not only that electric currents may be employed so as to produce movement in perceptible masses of matter, but that the motion is produced and maintained with a considerable degree of force,—that we really could get motors constructed so as to act by electric currents which would produce effects on a practical scale. But I want if possible to show you a little more than I have done as yet of the conditions under which such movements can be produced, and, in the first place, perhaps I may remind you for a moment of the first experiment that I showed you, the rotation of this little glass ball on this circular railway. We readily get that rotation maintained by turning the handle of this machine; but, if all we wanted was to get the ball to run round the circle, it clearly would have been much easier and would have required a much smaller expenditure of labour, to push the ball round, rather than to turn the machine and make it rotate in that way. So, if the only object were to get the ball to run round, there was a great waste of labour in the method we employed; but if our object was not merely to make it go round, but to make it go round under particular conditions, then the method we employed was perhaps as good as any we could use. What I want you to notice is that we did not get the

motion here for nothing. We had to expend labour in turning the electric machine, in order to get the ball to run round the railway. In all the other cases, when we get motion produced, we must either do work ourselves, or something else must do work for us; we may employ various agents to produce the electric current, but we cannot get an electric current which will give motion to these machines for nothing, any more than we can get electricity which will keep this ball rolling for nothing. We must pay for it either in work or materials in some kind or another. And not only must there be some expenditure to maintain the electric current, but the very movement which the electric current produces tends to cause a current in the opposite direction. In any of these machines which you have seen in motion, the movement of the machine would, if it took place by itself, produce an opposite current to the current which actually causes the motion; and I want to try to show you this in one or two cases.

I will try first of all to show you that when this little magnet spins, a current is produced by the spinning. There is a little patch of light on the screen, which is produced first of all by the light inside the lantern, but the light from that is thrown from a reflecting prism upon a little mirror in the apparatus in front, called a galvanometer, an instrument employed to detect the passage of an electric current. It would be beside my purpose to enter upon the principle of that, but I may just mention that there is a little magnet very finely suspended, so that it can move with the action upon it of an exceedingly small force, and attached to the magnet is a light mirror which is throwing a beam of light here on the screen. If the magnet moves, the mirror moves with it, and the little luminous patch will take a different position on the screen. If an electric current, even a small one, traverses the wire which surrounds the suspended magnet, there will be a movement of the mirror. In the particular experiment I want to attempt, the current I expect to get will be exceedingly small. So that if I get any motion at all, I shall be contented; you must not expect to see the thing moving violently, but I hope you may see a small movement. When I spin the magnet, there is a slight movement; and if I spin it in the opposite direction, the movement

of the spot is inverted, showing that the movement of the mirror is due to the motion of the magnet. You saw at the beginning, that a current passing into the instrument by means of wires causes the magnet to spin, and now we see that spinning the magnet causes a current to pass. It would be possible if we had time, to vary the experiment, and prove to you that the current which the spinning of the magnet produced is in the opposite direction to the current which would cause spinning in this same direction. If you make the magnet spin so that, as we look down upon it, it appears to go like the hands of a watch, we get a current in the wires opposite to the one which would cause the same kind of movement. And, therefore, when we put a battery, instead of a galvanometer, in connection with these wires, the battery has not merely to produce a current, but it has to overcome the current due to the motion of the magnet. This motion of the magnet if it were produced by any other cause, say by one's fingers spinning it, would produce a current; the battery has to destroy that current, and to produce one in the opposite direction, in order to maintain the motion. Again you saw that the same current can magnetise two pieces of iron at the same time, and I might have used instead of two pieces a dozen; the same current could have magnetised them all at once, and that without any diminution in the current itself, supposing that the conductors had been thick enough; or rather I should say the same current can maintain magnetism in any number of magnets without being diminished. But there is a difference between maintaining magnetism which has once been produced, and producing magnetism in iron which was previously unmagnetised. When a current first magnetises a bit of iron, there is an action produced, which tends to cause a current in the opposite direction. I will attempt to prove this to you.

Instead of employing an electric current to magnetise these pieces of iron, I will employ a steel bar magnet. If I put this against the piece of iron it will magnetise the iron for the time being, and I hope to be able to show you that if I magnetise one of these pieces of iron, by putting this magnet upon it, a current will be produced in the coil, and that current we can make to act

upon the galvanometer. As soon as I put this magnet upon the iron, we get a much greater action than we did just now. The spot of light is sent right off the screen to begin with, then it gradually comes to rest. When the iron there is magnetised, we have a current in one direction, in the surrounding coil of wire, and when it is demagnetised by taking away the magnet, we shall get a current in the opposite direction. When the spot of light has come to rest, I will remove the magnet, and you will see the spot goes right off on the other side; then it will gradually come to rest again. We may have ever so strong a magnet here, but it will not produce any current in the coil, as long as it remains in the same condition, but we do get a current if the magnetism is increasing or decreasing. As long as the piece of iron inside of these coils retains the same magnetic condition, we have no current at all in the coil, but if the magnetization of the iron is increasing or decreasing, we get a current in the surrounding wire, the so-called induced current.

Now in any of the arrangements already described whereby we can get work done continuously by means of electric currents, we must have alternate magnetization and demagnetization. That can be illustrated equally by means of any of these apparatus. In the first case, you must have this magnet active as the piece of iron is approaching it, and then its magnetism must cease as the piece of iron is moved away; otherwise, whatever work was done by the magnet in pulling the piece of iron towards it would be undone in pulling the iron away from it. What we want is that the magnet shall attract the iron as long as it is approaching, and allow it to move away freely as soon as it has come as close as possible. If the iron is pulled equally as it approaches the magnet and as it recedes, the advantage we get by the pull during approach is exactly neutralised by the resistance during separation. The range of motion is limited. As soon as the piece of iron gets close to the magnet you get no further motion, you must take it away again, so as to enable it to be attracted a second time, or,—to make the action still more continuous,—in most of these arrangements, we have several pieces of iron attracted in succession, and each one after getting as near as it can, must be allowed to go away again, to come back

sooner or later to the same place, so that we have in all these cases, magnetization and demagnetization, as a necessary condition for continuous movement. The effect of this is that we get a continuous succession of inverse currents,—or we may express the action in this way: While the machine is in motion, it produces a continuous succession of currents, opposed to the current which is causing the motion, so that the strength of the exciting current is diminished by the movement.

By way of attempting to show you this, I will make an experiment by means of that apparatus again, putting into the circuit a piece of platinum wire. I will allow the current which drives the engine to pass also through this bit of wire, and the effect of the passage of the current will be to cause the wire to be strongly heated. The action of the battery on the wire alone, without allowing it to act on the machine at the same time, would cause the wire to become luminous, but when it is acting on the machine the effect is much less. You notice now the current passing through the wire, and there is no visible heating of it, but the moment I stop the engine it becomes red-hot. That shows you what the strength of the current would be if the engine were not turning. If I allow the engine to turn, the current is weakened by the inverse action of the machine itself. I said that in any such arrangement as this, there is an inverse current produced by the motion of the machine itself, and that inverse current is sufficient to prevent the glowing of the wire; the temperature of the wire indicates the strength of the current we are dealing with; that is, it is a strong current when the machine is at rest, and not so strong when the machine is at work; and we get no visible heating effect. This shows that the movement, which the current produces, actually diminishes the strength of the current itself.

Another experiment which will further illustrate the same fact, or rather a variation of the same experiment I can make by means of these two apparatus. There are here two of those very beautiful machines for producing electric currents by mechanical action of the form devised by M. Gramme. By turning this handle we can, by an arrangement of electric conductors, soft iron, and a magnet, cause a continuous succession of electric currents. I

will try, first of all, to show you that by means of this apparatus you can get an electric current. I employ the same test we have used just now, the heating of the platinum wire. You see this is a sort of reversal of the effects we have produced by the other machines; in all those cases we employed an electric current to produce mechanical rotation. Here we employ the rotation to produce the electric current. I might produce exactly the same effect by means of a second machine which is almost identical except that it is turned by the foot instead of the hand; and by means of the two machines, I can illustrate still further the relation of mutual convertibility that I have just referred to; that we can produce motion by means of an electric current, or can produce an electric current by means of motion. If I turn the handle of the machine you see we get a current, or if I turn this second machine I shall be able to get a current as well, and I can use the one current to turn the other machine, and connecting the two machines together I can cause one of them to work the other.

Now, with these two Gramme's machines we will try the same sort of experiment I did just now; that is, we will include the piece of platinum wire in the circuit. The two machines are in connection with each other, as before, only the handle of one of them is not allowed to turn; and now I will try to make the wire hot again, and directly it is hot we will leave the handle loose. As soon as we allow the other machine to turn, you see the heat of the wire diminishes. A further extension of the same experiment is this. You see the direction in which that machine turns when we leave it free. I will ask Mr. Chapman, instead of merely holding it fast, to turn it in the opposite direction to the one in which it wants to turn, and then, instead of the wire getting less hot than before, it will get more brightly heated. I am using this machine to turn that one. If we have one machine turning, and connected with the other, and then turn the other one in the opposite direction to that in which it tends to move, we get an increased strength of current. But if we allow the second one to turn in the natural direction, the current is weakened. If we oppose the motion it tends to produce, we get a strengthened current. You

see now, when the machine is turned in the opposite direction to that in which it wants to go, the wire is brighter than ever; in fact, it is actually melted by the heat of the current.

The general condition that we have in all these cases may be thus stated: that the current is weakened by the motion it tends to produce, and it is the more weakened the more rapidly the machine which is employed as the source of power turns; further, the proportion of the whole working power of an electric current which is converted into useful work by any such arrangement as these, or any other, in fact, is greater the more the current is weakened. Let us imagine the current of a battery to have some definite strength when the machine is at rest, and some other definite strength when we have the machine going. The battery current is diminished by the inverse current due to the machine; so that the current which remains is the difference between the current of the battery and the inverse current due to the apparatus. If we call the current of the battery C, and the difference between it and the inverse current due to the machine, that is, the current which passes when the machine is in motion, C', then, without spending time on this somewhat mathematical point, I may just give it you as a matter of fact that the measure of the efficiency of the machine is represented by the formula $\frac{C-C'}{C.}$ * The proportion of the whole energy developed by the battery, which is converted into useful work, is the same as the proportion which the difference between the current when the machine is at rest and the current when it is at work bears to the current when the machine is at rest.

It is impossible, in consequence of these facts, to hope, from the application of electric force, for any essentially new source of power. We occasionally see statements that there is no reason to be afraid of the exhaustion of our coal-fields, and so on, because, when we can get no more coal to drive our steam-engines, we shall discover some new source of power, the usual hope being that electricity will be turned to some available account. Of course, one can never say what will be discovered, but we can say

* See Note (A).

what has been discovered, and so far as our present knowledge of the laws of electric currents or the laws of electricity in general goes, it does not indicate any means by which, when our coal is exhausted, it will be possible to obtain motive power through the agency of electricity. You see we can thus get motive power, but it is at considerable cost. We have to employ a galvanic battery, that is zinc, or some other metal, and acids in which the metal is dissolved. We have the solution of the zinc in acid instead of the burning of coal under the boiler, and the solution of zinc in acid is, quantity for quantity, a more expensive process a great deal than the burning of coal under a steam-engine boiler. The quantity of coal which must be employed to smelt the zinc to get it from the ore into the metallic state in which it can be used in a battery, would go much further as a source of motive power, if it were employed to produce heat under the boiler of a steam-engine than the zinc would go, if employed in a battery to give motion to such an engine as one of these. In fact, the experiment I drew your attention to first of all is the type of all the processes we can employ in getting motive power from electricity. To keep that little ball rolling on the railway, we must do much more work in turning the handle of the electrical machine than would suffice to keep up the motion of the ball. So here, much more work must be done in the way of coal expended in preparing materials for the battery than we get from the battery when it drives one of these machines. Or if, instead of using a galvanic battery, we use one of the Gramme's machines, we have to do more work in turning the handle of the machine than can be got out of the apparatus driven by it. We always have to expend, in one way or another, more work than we get out of any of these machines. Therefore, although we can get mechanical work done by them, we cannot look upon them as a possible source of motive power, when those we have at present at command shall fail us. (See Note B.)

The CHAIRMAN: Ladies and Gentlemen,—We have listened to a very instructive lecture, and I now call upon you to signify your appreciation of what you have heard by passing a hearty vote of thanks to Professor Carey Foster.

Note (A).

On the Efficiency of Electro-magnetic Engines.

The following additional particulars in relation to the efficiency of electro-magnetic engines may be of interest to some readers.

In the case of any machine which is working steadily, the energy expended on it in a given time exceeds the useful work done by it in the same time. Thus, in a steam-engine, the energy corresponding to the amount of fuel burned,—which is the mechanical equivalent of the heat given out in combustion,—is always greater than the amount of mechanical work done by the engine; the work spent during a given period in driving a machine-tool of any kind exceeds the useful work done by the machine during this period; and similarly, the energy equivalent to the consumption of material, in a galvanic battery used to drive an electro-magnetic engine, is greater than the energy given out by the engine as motive power. The proportion which the useful work, performed in a given time by a machine working at a constant speed, bears to the whole amount of energy expended on the machine during this time, is called the "efficiency" of the machine in question.

In the case of an electro-magnetic engine, driven by the current of a galvanic battery, the energy spent in a given time may be calculated as follows:—Let z be the quantity of zinc (or of some other material taking part in the chemical process of the battery and chosen for reference) which is consumed in the whole battery in unit of time, and let θ be the thermal effect of as much chemical action as goes on in the battery during the consumption of unit mass of zinc (or other selected material). Then the thermal effect of the total chemical action of the battery during unit of time is $z\,\theta$, and if J denotes the mechanical equivalent of the unit of heat, the expenditure of energy in the battery per unit of time is

$$W = J\,z\,\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Put ζ for the so-called "electro-chemical equivalent" of zinc, that is for the quantity of zinc consumed in each cell of a battery,

while the unit quantity of electricity passes any given part of the circuit,—then, if there are n cells in the battery, the quantity of zinc consumed in the whole battery, for each unit of electricity traversing the circuit, is $n\zeta$. Consequently, the amount of electricity which traverses the circuit during unit of time, or the *strength of the current*, is

$$C = \frac{z}{n\zeta}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and the quantity of zinc expended simultaneously is

$$z = C\, n\, \zeta.$$

Putting this value of z into equation (1), we get

$$W = n.\, J\, \zeta\, \theta.\, C \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

Of the quantities on the right-hand side of equation (3), J and ζ are necessarily constant, and θ depends only on the nature of the chemical action which takes place in the battery; for a battery of a given kind θ is therefore also constant. Consequently if we use a single symbol E to denote the product of these three factors, writing, that is,

$$E = J\, \zeta\, \theta, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

E represents a quantity which is characteristic of the special kind of cells composing the battery and is called the *electro-motive force* of one of these cells. If we have only a single cell, and if this produces a current of unit strength, the factors n and C in equation (3) each become unity, therefore, in this case, E = W or the electro-motive force of a given cell is equal to the amount of energy expended in it in a unit of time when it is traversed by a current of unit strength.

If we suppose the engine to be prevented from turning, the expenditure of the energy W in the battery has for its only result the production of a quantity of heat $H\left(=\frac{W}{J}\right)$, which goes to raise the temperature of the whole circuit traversed by the current. The heat produced in unit of time depends on the strength of the current and on a quantity called the *resistance* of the circuit, which is determined by the materials and dimensions of the various conductors, metallic or liquid, of which the circuit is made up.

Denoting the resistance by R, we have, as the result of experiment,—

$$W = J H = C^2 R \quad . \quad . \quad . \quad (5)$$

Combining this with equation (3), and putting E for its value from (4), we have

$$n E = C R \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Now let the engine be allowed to turn. As pointed out in the lecture, the strength of the current is in this case less than before; let it be represented by C'. Part of the energy of the battery is still spent in producing a quantity of heat H' in the circuit, such that $J H' = C'^2 R$; but, in addition to this, a certain quantity of useful work (= U) is done in maintaining the motion of the engine. Accordingly, denoting the energy expended in the battery per unit of time by W', we have

$$W' = C'^2 R + U, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and the efficiency of the engine is represented by the fraction

$$\frac{U}{W'},$$

whose value may be determined by aid of the following considerations:

Any electro-magnetic machine tends to move in such a direction as to increase the magnetic force acting across the area enclosed by the circuit of the electric current; and, in any given case, if a very small displacement, x, of the machine causes an increment of the magnetic force thus acting, which, when multiplied by the number of times that the area in question is encircled by the current, is denoted by m, the force tending to cause this displacement, when C' is the strength of the current, is equal to

$$C' \frac{m}{x}$$

The ratio $\frac{m}{x}$ depends on the construction of the particular machine, and it may either be constant, or it may go through a series of recurrent values once or oftener during each revolution. We will represent it by A. Then the work done during the dis-

placement x, or the product of the force into the displacement, is

$$A\,C'\,x.$$

Let a be the velocity of the machine and t the time occupied by the small displacement x: we have then $x = a\,t$, and the useful work done during the short time t becomes

$$U\,t = A\,C'\,a\,t.$$

Multiplying equation (7) by t, and substituting for the last term the value just found, we get

$$W'\,t = C'^2\,R\,t + A\,C'\,a\,t.$$

In general, neither the rate of expenditure of energy in the battery denoted by W', nor the strength of the current C', nor the factor A, remains constant for any finite length of time during the motion of the machine. They, however, pass through a series of values which recur periodically once or oftener during every revolution, and we may consequently take the symbols in the last equation as representing constant mean values. With this understanding, we divide each term by t, or, what comes to the same thing, we may regard the equation as referring to the energy expended and the work done in unit of time, and may write

$$W' = C'^2\,R + A\,C'\,a \quad . \quad . \quad . \quad . \quad . \quad . \quad (7a)$$

By (3) and (4) we have also

$$W' = n\,E\,C'$$

whence

$$n\,E = C'\,R + A\,a \quad . \quad . \quad . \quad . \quad (8)$$

and

$$\text{Efficiency} = \frac{U}{W'} = \frac{A\,a}{n\,E} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Substituting here the value of Aa from (8) and that of nE from (6), we obtain for the efficiency of the machine the value given in the lecture, namely—

$$\frac{U}{W'} = \frac{C-C'}{C} \quad . \quad . \quad . \quad (10)$$

The same result may be arrived at more readily by considering that the effect of an increase of the magnetic force acting across the area of the circuit is to generate, while the increase is going

on, an electro-motive force of the value $-\frac{m}{t} = -\mathrm{A}\,a$. Hence, while the machine is moving, the mean effective electro-motive force, or the electro-motive force which acts to maintain the current is

$$n\,\mathrm{E} - \mathrm{A}\,a.$$

But, by (3) and (5), the effective electro-motive force multiplied by the strength of the current gives the energy spent in unit of time in generating heat in the circuit, or

$$(n\mathrm{E} - \mathrm{A}a)\mathrm{C}' = \mathrm{J}\mathrm{H}' = \mathrm{C}'^2\,\mathrm{R}$$

and consequently the useful work per unit of time must be equal to the excess, above the energy so spent, of the total energy of the battery per unit of time, that is,—

$$\mathrm{U} = n\mathrm{E}\mathrm{C}' - (n\mathrm{E} - \mathrm{A}a)\mathrm{C}' = \mathrm{A}a\mathrm{C}'$$

and

$$\frac{\mathrm{U}}{\mathrm{W}'} = \frac{\mathrm{A}a\mathrm{C}'}{n\mathrm{E}\mathrm{C}'} = \frac{\mathrm{C}-\mathrm{C}'}{\mathrm{C}}$$

as before.

Expressions (9) and (10) show that the efficiency of a given electro-magnetic engine increases,—in the first place as its speed, a, becomes greater, and in the second place as the ratio of the strength of the current, C', when the machine is at work, to the strength of the current, C, when the machine is at rest, becomes smaller, and that the efficiency of the engine would have its greatest possible value, namely unity, either when the speed is great enough to make $\mathrm{A}a = n\mathrm{E}$, or when the strength of the current C' is equal to nothing. These two conditions are, however, shown by equation (8) to be identical, and if they occurred the engine would be doing no work, though it might still be called perfectly efficient, inasmuch as, there being no current, there would be no expenditure of energy in the battery.

For the actual rate at which useful work is done by an electro-magnetic engine, equations (1), (10), and (4), give the following expression—

$$\mathrm{U} = \mathrm{W}'\,\frac{\mathrm{C}-\mathrm{C}'}{\mathrm{C}} = \mathrm{J}\,\theta\,\frac{z}{}.\,\frac{\mathrm{C}-\mathrm{C}'}{\mathrm{C}} = \mathrm{E}\,\frac{z'}{\zeta}.\,\frac{\mathrm{C}-\mathrm{C}'}{\mathrm{C}}$$

Remembering that the strength of the working current C' and

the quantity of zinc z' consumed in unit of time are, according to equation (2), directly proportional to each other, it can easily be shown that with a given galvanic battery, an electro-magnetic engine is doing useful work at the greatest possible rate when it is working at such a speed as to reduce the strength of the current to half what it would be if the engine were at rest. In this case we have

$$U = \tfrac{1}{2} J \theta z';$$

or, numerically, supposing a Daniell's battery to be used, and taking as the unit of work the work done when a force, which would in a second give a velocity of a centimetre per second to a gramme, acts through one centimetre—a quantity of work often referred to as one *erg*,—

$$U = \tfrac{1}{2} \times 42{,}000{,}000 \times 805\, z'$$
$$= 16{,}905{,}000{,}000\, z'$$

where z' is the number of grammes of zinc consumed per second in the battery. The rate of doing work which is spoken of as "1 Horse-Power" is equal to about $7{\cdot}46 \times 10^9$ ergs per second: consequently a Daniell's battery consuming 1 gramme of zinc per second, and employed to drive an electro-magnetic engine working under the conditions named, would do work at the rate of

$$\frac{16905000000}{7460000000} \text{ Horse-power} = 2\tfrac{1}{4} \text{ Horse-power (nearly)};$$

or, to do work at a rate equal to 1 Horse-power, it must consume about 0·444 gramme of zinc per second or about $3\frac{1}{2}$ pounds (Avoird.) per hour. A good steam-engine performing the same amount of work, would consume about an equal weight of coal, costing from $\frac{1}{40}$ to $\frac{1}{50}$ of the price of the zinc.

Note (B).

On the available Sources of Electrical Energy.

We meet with electrical energy in nature in the form of the charge accumulated in thunder clouds and in the form of earth-currents, but the application to practical purposes of the energy thus exist-

ing would be attended with even greater uncertainty and difficulty than the employment of the kinetic energy of the wind or of the tidal wave as a source of motive power. Practically, therefore, we have to consider only artificial sources of electrical energy. These may be classified under three general heads as follows:— Electrical energy can be developed (1) by doing work in moving matter against electro-static or electro-magnetic force; (2) by the expenditure of chemical energy, as in the galvanic battery; (3) by the expenditure of heat, as in the thermo-electric battery. When we are considering the possibility of applying electricity as a motive power, it is evident that the first-mentioned source of electrical energy may be left out of account, since the motive power gained, even in an ideally perfect case, would only be equal to what would have to be expended in order to get the electrical energy required to produce it. With regard to the second source, chemical energy, this is accessible to us on a large scale only in the form of unoxidised combustible matter, in other words in the form of coal. The various processes of metallurgy are merely methods of transferring part of the chemical energy of coal to metallic ores. It follows then that, in the absence of coal, chemical energy would not be forthcoming, on a large scale, as a source of electrical effects. As to the third source, it may be sufficient to point out that we are again dependent on the combustion of coal for our chief supply of artificial heat. It is conceivable that some time electricity may play an important part in rendering the energy of the sun's rays, or perhaps in exceptional cases the internal heat of the earth, available as a source of energy for practical purposes; but it is certain that no discovery tending in this direction would restore to Great Britain her present exceptionally advantageous position as a manufacturing country, should her coal-fields ever come to be exhausted.

ANTARCTIC EXPLORATION.

By Captain J. E. Davis, R.N.

August 5th, 1876.

Major Festing in the Chair.

The Chairman: Ladies and Gentlemen,—I do not think any introduction of Captain Davis is needed to-night. I dare say many of you were here last Saturday evening when Captain Davis gave you such an excellent lecture on the North Polar discoveries; and he is now going to tell us about what has been done towards the South Pole.

Captain Davis: The attention of crowned heads and governments has from a very early period been directed towards the North Polar regions. The proximity of the Northern Pole to all the countries where science and commerce were concentred, with the prevailing belief of the existence of a passage whereby ships might make a speedy passage to India and Cathay, and so avoid the long, tedious, and dangerous voyage round the much-dreaded Cape of Storms, at once rendered the North an object of interest, not only in a scientific, but in a commercial point of view; and although the practicability of such a passage has long been set at rest, still ships have from time to time been sent to those inhospitable regions with no other object than geographical discovery and scientific research, while the South Polar regions have been comparatively neglected.

That this has been the case I think will be generally conceded, and also that our knowledge of what has been done in both

regions is far greater of the North than of the South, and the latter has never held the same prominence before the public as the former; and while the names of Parry, Franklin, Ross and a hundred others are "familiar in our mouths as household words," but comparatively few have even heard of Bellingshausen, Weddell, Biscoe, or Balleny; and even the renowned Cook is more remembered for his discoveries in the Pacific than for his bold pushing to the South and sailing over 102° of longitude within the latitude of 60°, 500 miles of which were within the Antarctic circle; and even now had it not been that the planet Venus crosses the sun's disc in 1882, to observe which a position in a high Southern latitude is considered the best for one of the methods of observing, the poor Antarctic regions might have remained neglected for another century or two, and every person connected with discovery in that part of the world forgotten. Under these circumstances I believe that a sketch of the history of discovery in the South Polar regions will prove both interesting and instructive On the early voyagers and discoverers in the South I must touch but briefly, in order to dwell more fully on the more recent and important ones.

The first voyage on which discoveries were made south of Cape Horn was that of one of five Dutch ships, fitted out at Rotterdam in 1599. The "Good News," commanded by one Dirk Gerritz, discovered the land known as South Shetland; La Roche discovered South Georgia in 1675, and Kerguelen, a Frenchman, the island known by his name, or by some as the Island of Desolation of Cook; the last named two islands are comparatively not in a high latitude, the first being in 54°, the last in 49°, still they were important discoveries where so little was known. The Sandwich group are believed to have been discovered in 1762, and have since been seen by various navigators. Auckland Island was discovered by Bristow in 1806, Campbell Island by Hazleburgh in 1810, and Heard and McDonald Islands by Captain Heard of the United States' ship "Oriental," and by Captain McDonald of the English ship "Tamarang."

Cook made no discoveries in the South, although he sailed over such a vast space, but his voyage was of this consequence,

in the parallels of latitude he sailed along we knew that for any large tract of land we must look further South.

The expedition of Bellingshausen in the two Russian vessels "Vostok" and "Mirni" was in like manner not of so much consequence from its discoveries as from its non-discoveries. He, like Cook, sailed through a great many degrees of longitude in which no land was seen, although he did discover two islands in a higher degree of latitude than any then known, namely, Petra and Alexander Islands, both in about latitude 69°; these were discovered in January, 1821. The highest latitude obtained by Cook was 71° 15′ on the 30th of January, 1774. Bellingshausen's highest was 70° on the 9th of January, 1821.

In 1818, a Mr. William Smith of Blyth, on his passage round Cape Horn, re-discovered the land known as South Shetland, also some land to the southward of it; on his arrival in harbour he reported his discovery to the English admiral, who sent the small vessel attached to his flagship, in command of Mr. Bransfield (the master of the flagship) to examine the locality. Mr. Bransfield confirmed Mr. Smith's discovery and added to it a portion of land which he called "Bransfield land."

The South Orkneys were discovered by Captain George Powell, in the sloop "Dove," on the 6th of October, 1821.

We next come to our countryman Weddell, an officer in the Navy, who left England in December, 1822, with two small vessels, the "Juno" and the "Beaufoy," one 160 tons burthen, the other a small cutter of 65 tons; this was a commercial expedition, the object being to obtain seal skins. In January, 1823, he crossed the Antarctic circle in longitude 30° West, and on the 20th February in nearly the same meridian he reached latitude 74° 15′, that being 185 miles further south than any navigator that preceded. "I would willingly," says that intrepid navigator, "have explored to the south-west, but considering the lateness of the season, and that we had to pass homeward through a thousand miles of sea strewed with ice islands, I could not determine otherwise than to take advantage of this favourable wind for returning." The gallantry displayed in this bold push south can scarcely be understood or appreciated

by those unacquainted with the nature of such a cruise, every mile of advance increasing in a compound ratio the risk in the return; and whatever may be the extent of our future discoveries in the south, there is not a doubt but that the name of Weddell will ever hold its own for gallantry and daring pluck.

The next voyage of consequence was that of Biscoe, also a commercial undertaking, fitted out by that spirited and enterprising merchant, Mr. Enderby. Biscoe left England in 1830, and in February, 1831 discovered land which had the appearance of being a continuous line of mountainous coast; to this he gave the name of his employer, and called it "Enderby land," and the next year he discovered that range of land called "Graham's land," on which he landed. In this voyage Biscoe passed over 160° of longitude within the parallel of 60°, 50 of which were within the antarctic circle. Many "appearances" of land are recorded in Biscoe's journal, but to his honour he would not map any but what he was sure could not be gainsaid.

All honour be to men like Weddell, Biscoe, and Balleny, these true pioneers of science, who without the appliances of government, with ships totally unfitted to the work, dared perils which a brave man with all those appliances might well hesitate to attempt, and here I would beg to digress for three minutes of the clock to apply a question. Why men such as these who bring so much honour on our country should nationally be unnoticed and unrewarded? The soldier or sailor dares the danger of a battle for a few hours, some even not in danger, yet all that survive are decorated with a medal, private as well as general, sailor as well as admiral, whereas in the pursuit of science let the sacrifice and danger be what they may, let one scale the lofty summit of the Himalayas, penetrate the icy regions to the very pole, subject himself to the influences of the deadly Niger, or devote a long and valuable life in the pursuit of some great discovery. What is his national reward? verily *nil*. They manage these things better in France, for while Leverrier for the discovery of the planet Neptune had honours showered on him by his government, those accorded to Charles Adams for the contemporary discovery by ours were—none.

Another small patch of land was discovered in 1833, near Enderby land, by Captain Kemp of the sealing schooner "Magpie."

An expedition of Mr. Enderby's ships in 1839 went south, the "Eliza Scott" and "Sabrina," commanded by Mr. Balleny, and discovered a group of islands in latitude 66¾°, on the 9th of February of that year; and a fortnight after they thought they saw the land, and a few days after, again the appearance of land was noted, to this last appearance he gave the name of "Sabrina land," and as these appearances of land agreed in position with part of that discovered next year by the Americans, there cannot be a doubt it was land.

In the years 1837 and 1838, two expeditions, one French and the other American, were fitted out for the purpose of discovery and scientific research; the former had for its commander the talented and unfortunate Dumont D'Urville, and the latter, Lieutenant and Commander Wilkes. The French expedition consisted of two ships, the "Astrolabe" and "Zebe," and the American of several vessels. These two expeditions pursued their voyage to different parts of the world, making important additions to the world of science.

About this time the illustrious Gauss and Weber turned their attention to the increasing importance of terrestrial magnetism: the synchronous perturbations of the needle at widely separated stations, even to the minutest vibrations, had caused magnetic observations to be established in various positions in Europe, but as these were confined to a small portion of the earth's surface, it became a desideratum to obtain a series of observations at distant and widely separated points; this was done and with the same result.

The newly organised British Association for the Advancement of Science, at this time took up the cause of magnetism, and through them the Royal Society; both bodies united in urging on the Government of the day the necessity of an expedition being sent to the southern regions, the principal object of which should be magnetism, by ascertaining the position of the south magnetic pole; the result was the fitting out of the "Erebus" and

"Terror." The command was given most appropriately to the discoverer of the North Magnetic Pole—James Clark Ross—than whom, I believe, to this day it is allowed, a better could not have been selected, at once an experienced Arctic navigator, a a good seaman, and a man of science. The second in command, Captain Crozier, lost his life as second in command in Sir John Franklin's ill-fated expedition to the North.

Gauss from theoretical calculations assigned a position to the south magnetic pole in latitude 66°, longitude 146° E. as an approximation; some asserted that the southern hemisphere contained two magnetic poles. This theory Gauss refuted, for he said that if there were two there must of necessity be three, as the needle from being vertical at one pole, must on its being transported to the other pole, have some intermediate point where the needle would be again vertical. However, on the former theory, Captain Ross received his instructions to proceed to the south in that longitude.

The "Erebus" and "Terror" sailed from England in September, 1839, left permanent magnetic observations at St. Helena and the Cape of Good Hope, remained three months at Kerguelen's land, and arrived at Hobarton in August, 1840. To Captain Ross's surprise he learnt on his arrival that the French and American expeditions, already mentioned, had anticipated his route and been south in the very direction they must have known he was ordered to proceed in. The American expedition, in doing this, carried out a part of the programme of their voyage already determined on, but no doubt both the commanders were inspired by an honourable emulation for their respective countries, to be the discoverers of the south, as England had been of the north magnetic pole.

In 1838, D'Urville had proceeded south from Cape Horn and re-discovered the land I have already mentioned as having been discovered by Smith and Bransfield, and also an extension of it which he carefully mapped and named "Louis Philippe," and "Joinville lands."

The second voyage south of this expedition took place in January, 1840. On the 1st it left Hobarton, proceeding south in the direction

of Gauss's magnetic pole and discovered land on the 19th in the latitude of the antarctic circle, this land was traced 150 miles and D'Urville named it "Terre Adélie." Proceeding westward he discovered about 60 miles of solid ice which he believed to be but a covering of the land; he named it "Coté Clarie." After suffering much from the weakness of their crews, the two vessels returned to Hobarton after an absence of only seven weeks.

In justice to the French it must be remembered that the crews had suffered from the debilitating effects of a long cruise in tropical climates, nor were the ships sufficiently fortified to stand the enormous pressure of ice if they had got entangled in the pack, so that much praise is due for the perseverance they manifested.

We come now to the American expedition under Lieutenant Wilkes; and with regard to his discoveries, I think, it will be seen very clearly that the error into which he fell was one quite natural and pardonable in one who had had no experience in polar regions, viz., that of mapping appearances of land as land, where the illusion is so great as often to deceive those who have had experience, and perhaps I might add, the wish being father to the thought of being a great discoverer in those regions helped the illusion, and had not that gentleman so persistently claimed all the land he mapped as *bonâ fide*, much allowance would have been made and much unpleasant comment would have been avoided.

The American expedition sailed from Sydney on the 26th of December, 1839, and on the 13th January, 1840, "something like distant mountains was thought to be discerned to the south-east," and a range of hills of about 50 to 60 miles was mapped; a nearer approach was impossible, in consequence of an icy barrier. Skirting this barrier to the westward, on the 16th appearances, believed at the time to be land, were visible, and Mr. Wilkes claimed the discovery of the Antarctic continent from that date. On the morning of the 19th, one hundred miles to the eastward in Mr. Wilkes's opinion, the land was certainly visible, and this opinion was confirmed by some of the oldest

and most experienced seamen on board. Mr. Wilkes says, "There now being no doubt in any mind of land, it gave an exciting interest to the cruise." On the 23rd, the appearance of land was observed to the eastward, near the spot inferred by Gauss as the position of the magnetic pole; the dip of the needle was found to be 87° 30′. On the 28th and 30th they had the land then in plain view, and on the latter day they approached within half a mile of the dark volcanic rocks in Piner's Bay, the land gradually rising beyond the ice to the height of 3000 feet, covered with snow, and extending east and west of their position fully 60 miles. This was in latitude 66° 45′, longitude 140° 2′ E.; all being then convinced of the existence of land, it was named the Antarctic continent. On the evening of the 8th of February, the outline of the continent appeared distinct though distant, and on the 10th, in longitude 120° E. they saw an appearance of land, although indistinctly; this was Sabrina land. On the 12th and 13th, land was distinctly visible 3000 feet high, and then none until the 17th, when appearances of land were seen, and this terminated the discoveries of the American expedition. The whole distance along the Antarctic continent considered as explored being 1500 miles.

Lieutenant Wilkes in his narrative says, "The credit of these discoveries has been claimed on the part of one foreign nation, and their extent, nay actual existence, called into question by another;" and again, "Each of these nations, with what intent I shall not stop to inquire, has seemed disposed to rob us of the honour by underrating the importance of their own exertions and would restrict the Antarctic land to the small parts they respectively saw."

Without delaying to consider whether 600 miles of connected coast with upwards of 400 miles of everlasting ice may be considered a small part, I will briefly examine Mr. Wilkes's title to the discovery of the Antarctic continent. There can be no doubt that the foreign nation that claimed the priority of discovery referred to is the French, who discovered Adélie land on the 19th of January, but I distinctly dispute either being the discoverers, and

claim it for our own country and due to our own countryman Balleny, who in the preceding February had discovered the islands that bear his name, and in March that named by him Sabrina land; this latter discovery being confirmed by Wilkes himself. Therefore, in respect to the priority of the discovery of the continent (if it be one), that puts both the foreign claimants "out of court." And if—as Mr. Wilkes is satisfied is the case—the land he discovered continues in an uninterrupted line to Enderby land, then, by his own admission, to Biscoe is the honour due.

If the extent, nay the actual existence, of his discoveries are questioned, Mr. Wilkes has only his own evidence to blame for it; as to the actual existence, I do not know where that has been called in question, but that considerable doubt exists as to what is land and what is not of what he mapped is proved by the fact that many geographers do not insert it on their maps, and we cannot feel surprised that such is the case, considering the grounds on which the land was laid down.

Mr. Wilkes considered himself justified in placing the first portion on the chart, because "something like distant mountains was thought to be discerned;" then another portion was added, because "appearances believed at the time to be land" were seen. Again, on the morning of the 19th, in Mr. Wilkes's opinion land was certainly visible; with these opinions, which all imply a doubt, there was "*no* doubt of the discovery of land."

On the 23rd the appearance of land was observed, another insertion, and five days after Lieutenant Wilkes at last says: "We had the land now plain in view;" and on the 30th, "now that all were convinced of its existence I gave the land the name of the Antarctic Continent." Thus it will be seen that until the 28th there was always some doubt, and until the 30th all were not convinced.

I have carefully considered what may reasonably be granted to have been land, and what still requires confirmation, and have recorded both on the chart now before you.

In justice to the American commander, it must be observed that his ships, like the French, were totally unfitted by want of the necessary fortification, for attempting to press through the icy

barrier he met with, and there can scarcely be a doubt had he done so, his ships would have been crushed, so that in commenting on the value to be attached to his discoveries, I have no intention of depriving him of the great credit due to his perseverance and energy, nor did he relinquish the exploration until obliged in order to recruit his exhausted crews.

After establishing a magnetic observatory at Hobarton, Captain Ross with the Erebus and Terror sailed on the 24th of November, 1840, visited Auckland and Campbell Islands, and then struck south in the 170th meridian of east longitude. On the 28th of December the first iceberg was seen in latitude $63\frac{1}{2}°$. The first sight of one of these enormous masses is most interesting; unlike those of the north they have generally nothing fantastic about them, but resemble huge twelfth cakes well sugared, while the sides are a delicate blue, increasing in the cracks and fissures to an intense cobalt. I do not hardly know how I can bring to your ideas what a large iceberg is, but I will attempt it. If you suppose a berg put down on the city of London, it would blot it out; that is to say, it must be 7 miles long by about 4 or 5 miles broad, and if you imagine the height of the monument, it will give you some idea of the height of an iceberg. But only to some extent, because you must recollect that only a comparatively small portion of an iceberg is above water; if it is 200 feet above the sea, it is 1400 or 1500 feet below. If then you attempt to calculate the number of tons of ice contained in such a mass, you will see that the figures would be very large indeed.

It is while sailing near and beneath one of these mighty works of creation, when the very breath is held in awe, the feeling of chill from the berg acting on the senses, and the consciousness that if a small portion were detached from the top, what the fate of the cockle-shell of a ship beneath would be, that the consideration steals over one of the insignificance of man's works, when placed in juxtaposition with those of his maker.

Proceeding south they encountered pack ice on the Antarctic Circle, and as this was the latitude in which both French and Americans had made land, it was supposed that the ships were

approaching land also, but Captain Ross at once pushed into the pack, and by dint of perseverance a distance of 200 miles was accomplished in three days. I may here remark that pack ice in the south consists of an accumulation of lumps and small masses, generally from 10 to 100 feet in thickness, interspersed with bergs, and of uncertain extent, the navigation of which is extremely difficult, and at times hazardous.

Great was the joy of our navigators at finding themselves in an open sea; the young and inexperienced now thought that nothing was to be done but to sail on and gain the pole itself; not so their elder and more experienced companions. They were as delighted as the young to see clear water, but knew full well the probabilities against reaching a high latitude; the consequence was, when any check was received the former were all disappointed and dejected, while the latter were as cool and calm as if they expected to meet that very difficulty in that very spot; and experience soon taught youth that it was not by impatience or rushing at anything that great ends were to be attained, but by steady perseverance and patience.

Shaping his course towards the great object of his voyage, the magnetic pole, on the 11th of January, 1841, at 2 in the morning, high land was seen distant 100 miles. Those who have experienced it, will know; to those who have not, it is impossible to describe the feelings with which one first views land on which the eye or foot of human being has never rested; the sensation of pleasure, excitement, and curiosity, then experienced can never be forgotten.

This the most southern known land in the world, appeared quite close in the morning, owing to its height and the extreme clearness of the atmosphere. On approaching they found it a mass of icy mountains, from two to 10,000 feet high, stretching to the southward and also to the north westward, the sea front being perpendicular glacial ice. An attempt was made to land that evening, but failed.

Proceeding to the southward, a landing was effected the next day on an island off the coast. I was one of the party that landed; we had much difficulty to get on *terra firma*, for a fringe

of ice had formed on the rocks, which prevented our getting a footing, as we were obliged to leap from the boat. However, we all got on shore without accident, and possession was proclaimed of the surrounding lands in the name of our most gracious Sovereign, after whom the main land was named, and the British flag waved over her most southern dominions, while three hearty cheers resounded on land where the human voice was never before heard, and which cheers greatly astonished a sturdy race of penguins by whom the island was inhabited, and who almost disputed a landing, and could well have done so had they known how to apply their energies, for they were a countless multitude ranged in rows on every ledge of rock from the summit to the water's edge. The weather appearing threatening, the stay was short and much to be done; every one was busy, the ornithologist was shooting birds, the botanist with eager eyes was searching every crevice of the rocks, hoping to find only one little lichen to reward his toil. Alas! he was disappointed, he had got beyond the bounds of the vegetable world; the magnetometricians were taking a few observations with their pet dipping needle. I was doing the same with my theodolite, whilst others were loading themselves and the boats with stones, penguins, etc.

The island was found to be entirely composed of igneous rock on which a glacier had formed, and which projected far out into the sea. A signal from the ships warned our party that a fog was coming down, which caused a speedy embarkation, and we were not too soon, for scarcely had we reached the ships when a dense fog enveloped them.

Proceeding south the land was traced, and on the 23rd Weddell's highest latitude 74° 15′ was passed, on which occasion the main brace was spliced.

On the 27th Franklin Island in latitude 76° was discovered and landed on, and the next day two mountains were seen to the south at a distance of 120 miles; from one, flame and smoke issued; this was 12,400 feet high, and was appropriately named Mount Erebus; its companion, 10,000 feet high, was named Mount Terror. The discovery of an active volcano in so high a latitude was extremely interesting.

From the eastern point of Mount Terror, Cape Bird, a perfect wall of ice, was seen stretching to the eastward far as the eye could reach; its appearance was uniform, without much indentation, its height varying from 150 to 200 feet; no land was seen behind or to the southward of it.

Great excitement was naturally caused by these discoveries, and great were the hopes entertained as the ships proceeded to the eastward along the face of this formidable barrier, that the corner would be reached round which they would again be able to proceed south; but hour after hour, and day after day passed, with increasing wonder in the minds of all, without reaching the corner, and after tracing it through 25° of longitude, pack ice prevented further progress. Meeting with this obstruction was a great disappointment. Here was a barrier, indeed, that seemed to say, "Thus far, and no further," and Captain Ross very graphically says of it: "It was of such a character as to leave no doubt upon my mind as to our future proceedings, for we might with equal chance of success try to sail through the cliffs of Dover, as to penetrate through such a mass." A remark he made at the time may give some idea of his opinion; comparatively, he said, that "until then he had never seen ice." After one day's sailing along it, I heard one of the men remark that the barrier "must be the grandfather of all the icebergs." The disappointment was general, although a latitude had been reached at 10 P.M. on the 4th of February (deduced from an observation of the sun under the pole, at midnight) of 78° 4′.

Retracing their steps the ships penetrated through several miles of new or pancake ice, into a bay westward of Mount Erebus; rather a dangerous proceeding, as, should the wind fail for a short time, the ships would have stood a good chance of being frozen in; but as it was the direct path to the magnetic pole, Captain Ross was naturally anxious to approach it as nearly as possible. On the 18th of February the dip of the needle was 88° 59′, or only about 160 miles from the magnetic pole. It may be possible to get a few miles nearer, but as the land was only 10 or 12 miles distant, and that land impassable in the direction of the pole, it is not probable that any one will ever effect a nearer approach to it.

Whilst in this bay many splendid views of Mount Erebus in eruption were seen, but not to advantage, for want of darkness.

Returning north to the point first discovered, the examination of the coast to the north-west was continued. Captain Ross was most anxious to discover a harbour in which to winter, and in his enthusiasm I heard him say that he would give his right arm to discover a harbour; but if there were harbours they were so blocked up with ice, and to a considerable distance seaward, as to be impracticable, so there was no alternative but to return.

On the 4th March Balleny islands were clearly seen. A gale springing up that evening, the ships were in a critical position, for having a tracing of Lieutenant Wilkes's chart on board, the continent mapped out by him was under our lee, but after an anxious night, to the surprise of all no land was in sight from the crow's nest, and at noon on the 6th the latitude and longitude of both ships placed them on the site with no bottom at 600 fathoms. This was the land "thought to be discerned," but, as I have already commented on it, it is only necessary to say that it must have been one of those illusive appearances of land alluded to.

The Erebus and Terror arrived in safety at Hobarton on the 6th of April, after an absence of 6 months, and without a man on the sick list in either ship.

Captain Ross had now the satisfaction of hearing that Professor Gauss had, during the absence of the ships to the south, recalculated the position of the magnetic pole, and placed it considerably to the southward of the position he had formerly assigned to it, and only a short distance from its actual position as discovered by Captain Ross. As the first voyage south of Captain Ross was the most important, in regard to discovery, I shall get over the other two more rapidly.

On the 23rd November, 1840, the ships left the Bay of Islands, New Zealand, and proceeded south, considerably to the eastward of the course taken the year before, or in the 150th meridian of west longitude; they encountered the pack in latitude 62° and entered it, but they were not fated to get so quickly through it as they did the preceding year, for in a few days the ice became

very heavy. From the 22nd December to the 2nd February, 41 days, they did not advance more than 200 miles; on that day, the 2nd, they cleared the pack, escaping many dangers.

In a gale in the pack which more resembled an earthquake than a storm at sea, the Terror had her rudder torn from her stern-post by a piece of ice, although the fastenings were those of a line of battle ship; the Erebus had the head of her rudder wrung. Although the season was far advanced, Captain Ross persevered, keeping along the pack edge until, on the 23rd February, the mighty barrier was sighted, and passing through a quantity of newly formed ice, the highest southern latitude ever attained was reached, viz., 78° 11′, this being 4° further than any of our own countrymen, and 8° further than any foreigner. This position was but a short distance to the eastward of the highest latitude of last year. The barrier at this point was more irregular, and there was a strong appearance of land, and which I believe was land.

It was a nice point, and a point of honour, as to which ship would be the most advanced, but the pancake ice so marked the track of each ship that after the Erebus, which was the leading ship, had tacked, Captain Crozier was enabled to put his helm down on the spot Captain Ross did, so there could be no dispute; one man indeed boasted of having been further south than his companions, as he was on the jibboom when the helm was put down.

It would occupy too much time to give you an account of the dangers and difficulties that had to be encountered before reaching this high latitude; to tell that at times when blowing hard, the spray (which only freezes at 28° when still) froze as it flew over the ships before it reached the decks or rigging, encrusting every part; that it occupied the seamen hours with axes and sticks to clear the blocks and ropes, to perform the most ordinary evolution; how that the strength of strong men was reduced to that of infants, from the uncertainty of their footing and hold; and all this in the very height of summer, when, if it had not been for the all protecting hand of Him "who stilleth the storms and the waves," all their efforts would have been indeed in vain.

Returning north with nights lengthening and days shortening, the navigation was extremely dangerous, thousands of bergs studded the seas that had to be passed through. Little can you who in retiring to your rest at night and whose greatest fear when the storm is loud, is that a few tiles or a chimney-pot may be blown off your dwellings,—little can you imagine the anxiety of one night in these seas, knowing not when you lie down if you will ever rise again, and feeling and knowing that a collision with one of these monsters of the pole would summon you in a very few minutes to the presence of your Maker. Some idea of the anxiety felt may be gathered from the fact that some of the seamen even deferred sleep night after night, snatching what hasty repose they could during the day, when the danger was not so great.

After being 136 days without seeing land, the ships arrived in a very battered condition, at the Falkland Islands. Having refitted and obtained a series of magnetic observations both at the islands and near Cape Horn, on the 17th of December Captain Ross again left for the south; but this season he selected the 55th meridian of West longitude in expectation of meeting a continuation of Louis-Philippe land. They met the pack, and, as was expected, a continuation of the land was seen, but trending to the southward; and on the 6th of January, 1843, Captain Ross landed on Cockburn Island, and on this land the most southern vegetation was found: the flora contains 19 species—all mosses, algæ, and lichens. The expedition was again beset in the pack 42 days, at times gaining a few miles of southing and then driven back. Most annoying was this to be literally making no progress, at the same time being kept in constant bodily exertion in extricating the ships and repelling the attacks of the ice, and living in continued alarm and anxiety.

On the 4th of February the ships were again clear of the pack, having added about 180 miles of coast line to the former discoveries. Proceeding along the edge of the pack to the eastward, with the wind against them, was tedious work in such dull sailing ships, as the season was far advanced and every hour was of consequence. On the 27th they again struck south in longitude 12° W. On the 1st March they again crossed the antarctic, on the 5th

they reached the highest southern latitude of that season, 71½°, when heavy pack ice and thick snowing weather forbade further progress, and compelled Captain Ross to turn the vessels' heads northward, for again willingly to have entered the pack at this late period would have been little short of madness. Again were experienced those dreadful nights of anxiety of which I spoke on the return from the south the year before. On the 11th the antarctic was recrossed for the last time, and many a heart with truth and fervour thanked God for that.

On the 4th of April they arrived in Simon's Bay, Cape of Good Hope; from thence touching at St. Helena, Ascension, and Rio Janeiro, they reached England on the 7th of September, after an absence of just four years, having in that time experienced many instances of God's mercy and adding very many stones to the mighty cairn of His unfailing providence.

The last voyage made to these desolate regions is that of H.M.S. Challenger the year before last. I need scarcely remind you that this ship, like the American and French expeditions of which I have spoken, was not fitted out for Polar discovery, and therefore not strengthened to resist the pressure of ice, and with such a ship it would not have been prudent to enter the pack. You will, however, see by the chart that she crossed the antarctic circle, but in going so far south it was not with the object of geographical discovery, but connected with the physical condition of the great ocean waters. She approached within 15 miles of Wilkes's "Termination land" without seeing it. Although in the American narrative this land was only supposed to have been seen, it was mapped as land.

In conclusion I may say that if the necessities of astronomers require a station in a high southern latitude, we shall, on the return of the Alert and Discovery from the North Pole, have a set of trained officers and men quite equal to the occasion of seeking one, and without which it would be almost an impossibility to attempt. That the necessity may not arrive and that the observations of 1874 will entirely set the matter of the distance of the sun from the earth at rest, is devoutly to be wished; but of all classes of men that I am acquainted with, philosophers are the

least satisfied. Like Oliver Twist they are ever asking for more, and if all the calculations of all the observers concur in establishing the exact distance, there are sure to be some who would wish to have that exact conclusion verified, or rather wish to discover some discrepancy in order that they might have an excuse for asking for more expeditions to observe the transit at the next opportunity—in a hundred years time!

The CHAIRMAN: You have already by your applause signified your thanks to Captain Davis, and therefore it is hardly necessary for me to ask you to pass a formal vote of thanks to him for his exceedingly interesting lecture.

SOME PROPERTIES OF GASES.

By Professor Herbert M'Leod.

August 7th, 1876.

Major Festing, R.E., in the Chair.

The Chairman: Ladies and Gentlemen,—I have to introduce to you to-night Professor Herbert M'Leod, of the Engineers' College, Cooper's Hill, who will give us a discourse on a subject which has, to some extent, been treated of by the Right Hon. Dr. Playfair, in his lecture on "Some Properties of Gas." I think there is no further introduction needed on my part.

Professor M'Leod: I am afraid this evening I am not going to show you any fireworks, but I will endeavour to give you an explanation of some of the instruments which are present in the Exhibition—some two or three only, because in this enormous collection it is impossible to select more than a very few to illustrate a single lecture.

Gases, although ordinarily invisible, have quite as real an existence as solids or liquids. If you take a bladder which contains a small or a large quantity of air, you feel, on squeezing it, that there is something in it. Now, that something you will not be able to see. If it were in a glass vessel you would not be able to distinguish it in the least by the ordinary process of vision. But it is capable of being weighed and measured, and experimented with in various ways. Like solids and liquids these invisible gases are attracted by the earth; that is to say, they possess weight, and I want to show you this. Possibly many of you have not seen air weighed, and I will endeavour to bring this actually

before you. Here is a glass vessel, which at the present time contains air, and it is suspended on a balance, and accurately counterpoised by some shot placed in the other pan of the scale. Now we will pump the air out of this glass vessel, and I think you will find it will weigh less than it did before; it will, in fact, become really lighter. I will now exhaust the air from it; and I think you will see this comparatively very small quantity of air which has been removed from it has been sufficient to upset the counterpoise of the balance. I will hang it on as before, leaving the shot standing as previously. Now you will see the glass vessel is lighter than before. Perhaps you may think there has been some little conjuring going on; but to show that is not so I will open the stop-cock and admit the air again into the glass vessel, and leave it entirely to itself, and you will see the balance has been restored to its original position. It is not quite gone back; but probably the balance, which has been a journey of five-and-twenty miles, is a little out of order. The air actually having weight, there must be a pressure of air on the surface of the earth. The earth must be pressed upon by the pressure of the atmosphere which is above it; and this actually takes place. You do not ordinarily perceive this; but here is a piece of bladder, which is tied over a glass vessel. You do not notice that there is any special pressure on the side of this bladder, because the air is pressing on both sides of it; it is pressing through the open vessel on the inner side, and on the outside of the bladder in an opposite direction; and these two pressures support one another. But if we remove one of these pressures in part, which we can do by withdrawing the air from the inside, then you will see an illustration of the effect produced by the pressure of the air on the outside. I put it on the plate of an air pump, and draw the air out, and the bladder is being bent down very distinctly. When I just touch it there is a slight explosion, and a rush of air inwards.

Now, the pressure of air which is acting on the surface of the earth, is measured by means of a barometer. That consists of a tube of mercury, closed at one end, and open at the other. Here is such a tube, filled with mercury to the top. I will close the

open end with my finger, and then turn it upside-down in the vessel of mercury, and then open it. When I withdraw my finger you see the mercurial column falls, and the mercury sinks to a certain extent. It is supported at that particular height by the pressure of air acting on the surface of the mercury contained in the trough. The trough here is exposed to the action of the air, and the air, pressing upon that, forces the mercury up the tube. The barometer was invented by Torricelli, who was the pupil of Galileo; and there is downstairs one of the original barometer tubes made by him. The mercury stands in this tube at a certain height above the mercury in the trough. I measure that height by a yard measure, and find it is almost exactly thirty inches. This, then, is the height of the column of mercury supported by the pressure of the air on the mercury outside. If you weigh a column of mercury, which has a square inch of area, and is thirty inches long; that is to say, if you weigh thirty cubic inches of mercury, it will be about 15 lbs.; and therefore on every square inch of the surface of the earth the atmosphere presses with a force of 15 lbs., and it is this 15 lb. pressure which burst the bladder.

We are not in the habit, in scientific work at the present time, of talking much about inches; but we make use of a measure which is used not only by Englishmen, but by a great number of others. I mean the metre measure. A metre is a little more than a yard; it is about 3 feet, $3\frac{1}{3}$ inches. Now, the height of a barometrical column we have found to be 30 inches, but it varies a little from this. The pressure of the air sometimes diminishes, and sometimes increases. When it diminishes the barometer falls, and the mercurial column becomes shorter; when the pressure increases the barometer rises, or the mercurial column becomes longer; but 30 inches is about the mean. The French mean, that is, the number which is taken as the ordinary mercurial column; for the atmospheric pressure in metres is ·760, or, as we may call it, 760 millimetres, a millimetre being one-thousandth of a metre. As we are talking about millimetres and metres, I thought it was as well to tell you the difference between such measures and inches.

The air which is on the surface of the earth is, of course, pressed upon by the weight of the air which is above; there being a considerable height of atmospheric air, in fact, something like 200 miles, above the surface, that which is below must be pressed upon very much by that which is above. But if you remove the pressure from the air, the air will expand; there will be nothing to keep its particles together; in fact, it will expand indefinitely. So far as we know, no one has as yet reached the limit to which air will expand when the pressure is removed. Here is a small bladder, containing a small quantity of air, and which, I hope, is tied up perfectly. I am going to put it into this receiver; and then, by the air pump, we will remove the air from the vessel surrounding the bladder, and therefore remove the pressure from it. It is now apparently filling, and if I can get anything like a perfect vacuum, the bladder will become perfectly tight. I am not pumping the air into the bladder, but simply removing the air from outside it. Now, if I let the air into the receiver again, the atmospheric pressure on the outside will be restored, and the bladder will shrink to its original size. This is an indication, not of any alteration in the size of the bladder, but of the air which was contained within it. So that you observe when the pressure is removed from the volume of air, that volume of air increases in size; and conversely, when you increase the pressure on a volume of air you diminish its size. This is also the case with solids and liquids; they may be compressed, but the amount of pressure which they require is something very considerable. I will just give you a rough experiment in order to prove this. Here is a flask which contains some water, made black with ink, so as to show it more plainly. Attached to it is a bladder containing air. If we squeeze the bladder it produces, of course, a pressure on the surface of the water; but the surface of the water remains apparently constant. It does not seem to diminish in quantity. Now, I will turn the apparatus upside down, and then I shall get the bladder full of water, and the flask full of air. I will put my thumb level with the surface of the column of water, the vessel above containing air, and the vessel below the water. And now, if I squeeze the vessel below containing the

water, I condense the air, or make it less, as you see by the rise of the water; but on removing the pressure the gas expands once more, so that, increasing the pressure on the gas, you diminish the volume, and diminishing the pressure on the gas you increase its volume. These variations of volume may not only be stated roughly, as I have, namely, that the increase of pressure diminishes the volume, and a decrease of pressure increases the volume; but the fact is very much more complete than this. The volume is diminished in the same proportion that you increase the pressure; and if you diminish the pressure the volume is increased in the same proportion that the pressure is diminished, or, according to the law which is generally given—which was discovered by Mr. Boyle, and which is called Boyle's law—the volume of a gas is inversely as the pressure to which it is subjected. That is to say, if you increase the pressure to a certain extent you diminish the volume; and if you decrease the pressure you increase the volume.

Let me see if I can show you this. In these two tubes are two gases. One tube contains common atmospheric air, and the other contains common coal gas—two gases as different as possible from one another. Each tube contains mercury at the lower part, and gas at the upper part; and the volumes of these two gases are equal. Now, these two tubes communicate, by means of a little India-rubber tube, with a reservoir of mercury. If I lower this reservoir of mercury, I shall diminish the pressure under which this gas exists at present. It at present exists under atmospheric pressure; that is, the air presses on the surface of the mercury, but the mercury inside this vessel produces a pressure on the mercury throughout the whole length of the tube, and causes the mercury to rise in the closed tube to this particular height, so that these gases are actually under atmospheric pressure. The column of mercury on one side of the India-rubber tube being exactly of equal height to the column on the other side, they counterbalance each other, and therefore there is simply atmospheric pressure acting on the gases in both tubes. Now, if I lower the mercury reservoir, some mercury runs out of the tube; but this will not go on indefinitely. First, I will measure the height of the column, or the length of the column of gas. The length of the

column is given roughly by means of this piece of string. I will put it at such a point that it will indicate exactly when we have doubled the volume, and that will be when the mercury has sunk down to a particular spot, which I will mark with my finger. I have now got the volumes of gas exactly double; and I want to measure the difference of height between the mercury in the tube and the mercury in the reservoir. I can do that sufficiently for this purpose, by placing the end of a rule on the level of the mercury reservoir, and putting my thumb at about the level of the mercury in the tube; and it appears that the column is only about 15 inches, or exactly half the 30 inches, which is the height of the barometer; so that by lowering this to a certain point we have doubled the volume of gas, but we have the column of mercury only 15 inches, instead of 30 inches, which was the height of the mercury column supported by the atmosphere, so that in order to double the volume of gas we have had to halve the pressure; that is, the volume is inversely as the pressure.

Now, let us take this in the opposite direction. Let us now diminish the volume of gas. Here is my piece of string, which is the proper length of the column; I will cut it in half, and reduce the gas to half its volume. In order to do that, we shall have to increase the pressure by raising the reservoir of mercury. Having done so, we will measure the length of this column of mercury; that is the difference between the level in the tube and the level in the reservoir; and now that is 31 inches. In this case we have rather overshot the mark, and have had to raise the mercury column a little higher than 30 inches, in order to halve the volume. If the experiment had been done with sufficient care—if the tube had been properly graduated, we should have found we had to raise the mercury column to 30 inches instead of 31. But this, for a lecture experiment, is perhaps sufficiently accurate.

This law of Boyle, that gases are inversely as the pressure to which they are subjected, was thought to be absolutely accurate for a long time, until a celebrated physicist of the present time tried numerous experiments on the compression of gases. This was a Frenchman, of the name of Regnault—and I am afraid I have a kind of infatuation for Regnault, because his experiments

were conducted with such wonderful care and precision. He experimented on several gases, and found the law of Boyle not absolutely correct. But it is very nearly so, for many gases. I think some of you have heard a lecture delivered here on Faraday's experiments, in which he compressed gases to a very small volume, and found a large number were converted into a liquid condition, merely by the squeezing of the particles together. Now, of course, all gases which are capable of being liquefied cannot entirely fulfil the law of Boyle; because the liquid occupies very much less space than the vapour from which it is produced. So that all gases which liquefy must be left out of consideration. But there are only six gases which do not liquefy, when they are compressed or when they are cooled; and these six are the following—three of them being well known to you.

Hydrogen, which is one of the gases existing in water;

Oxygen, which is the other gas existing in water;

Nitrogen, a gas which exists in atmospheric air; and in fact forms four-fifths of it, the other one-fifth being oxygen.

And then there are three compound gases. One is

Carbonic Oxide, which is the gas which burns with a blue flame on the top of a charcoal fire.

Then there is a gas called

Marsh Gas, which is the fire damp of the miner, a gas which, when mixed with air, produces explosions.

And the last gas is a

Compound of Nitrogen and Oxygen, know as *Nitric Oxide*, a gas which is produced when nitric acid acts on copper or other metals.

These six gases have not been liquefied up to the present time by any process to which they have been submitted. Regnault experimented on gases by means of a long column of mercury. He did not do it in the rough, or as we have done it here, by raising the reservoir of mercury connected by an India-rubber tube to the two glass tubes; but he used an apparatus of which there is what I may call a model in the next room, which I recommend you to examine for yourselves. It is not the original one of Regnault; but a modern model, though

made on very much the same principle. The tube into which Regnault passed his gas was three metres in length, a little more than 9 feet; and then he had a glass tube which contained mercury, which was 30 metres long, or about 100 feet; and this was erected against a tower. Of course it was impossible to get a glass tube 100 feet in length; and therefore he used a number of tubes connected together by iron joints such as I have here, which were ground together and cemented on the ends of the glass tubes, so that no mercury could leak out. This long tube of 30 metres high, was erected against the tower; and the gas was pumped into the closed tube of three metres high. There was a brass stop-cock on the top, which could be connected with the air pump, and at the bottom was an iron tube running horizontally, and passing up from that was the long tube of 30 metres, and beyond the long tube the pump by means of which the mercury is forced into the vessel. First by means of the air pump he forced the air in until the column of mercury came nearly to the lower end of the tube; then he pumped the mercury in until the column was reduced to nearly half of its original size, and then measured the height of the column of mercury. This was done, with great precision, by means of an instrument, many specimens of which are in the measurement room, an instrument called the cathetometer. It consists of a telescope placed on a sliding stand with graduations, through which you look at the surface of the mercury in the tube, and read off on the graduations the height of the column. Of course the cathetometer was arranged in such a way that the observer could read any portion of this high tube with great accuracy, to the $\frac{1}{100}$th of a millimetre. Regnault determined in this way that Boyle's law was not absolutely correct. He found that all gases, except hydrogen, are compressed more than they ought to be—that is, their volume is diminished more rapidly than it ought to be, if Boyle's law were true. If you take one volume of gas under the pressure of one atmosphere or about 760 millimetres of mercury, and if you want to reduce that volume of air to one-tenth, according to the law of Boyle, you would have to use a pressure of 10 atmospheres. But Regnault found that you had really to use only 9·92

atmospheres. So that 10 atmospheres would produce too much condensation. Then to reduce the volume to one-twentieth, you should use a pressure of 20 atmospheres; whereas Regnault found that 19·72 was sufficient, and that 20 would have condensed it too much. Then he tried another gas—carbonic acid gas, which is produced when charcoal burns in the air. That is a gas which may be liquefied by high pressure, as was discovered by Faraday. He found that if he took carbonic acid gas under pressure of one atmosphere, in order to reduce it to one-tenth of its volume, he had to use a pressure of 9·23 atmospheres, which is less than in the case of air, and when he reduced it to $\frac{1}{20}$th he had to use as little as 16·71 atmospheres. Whereas, according to the law of Boyle, it ought to have required 20. Then he tried hydrogen, and found that when he took a volume of hydrogen, measured at a pressure of one atmosphere, it required to reduce it to one-tenth, a pressure of more than 10 atmospheres, namely 10·06; so that you see hydrogen condenses less than it ought, and is not compressed so much as the other gases; or so much as would be indicated by the law of Boyle. In order to reduce this to one-twentieth he had to use a pressure of 20·27. Now, in a great number of the forms of pressure-gauge in which advantage is taken of the compression of air, the volume of air is enclosed in a certain vessel; and the diminution of this volume is employed as the measure of the pressure.

Bodies when heated, usually expand or become larger. Solids expand to a slight extent by the action of heat; liquids more so; and gases still more so. In order to show the expansion of a solid body, you require a very delicate apparatus. But there is one here on the table which belongs to the Freiburg Physical Institution, for showing the expansion of solid bodies by heat. There is an iron bar placed in a trough into which hot water or hot oil can be put, and one end is placed against a lever, which lever carries a looking-glass. Now the expansion of a solid object, such as a bar of iron, is so very slight that you must multiply the expansion very much, in order to be able to detect it at all. The bar is placed in ice first, and its length is measured by the position of the mark seen by reflection in the

looking-glass. Then it is heated, and as it gets hotter it lengthens very slowly, and moves the position of the looking-glass through a very small extent indeed; but just sufficient to give an indication on a scale placed at a considerable distance. You see, in consequence of the small increase in the length of the iron bar, you have to employ this mechanical contrivance to multiply the lengths, in order to enable you to see it all. Liquids also expand only comparatively slightly; but more so than solids. Here is a vessel that contains inky water. I will pour into this vessel which surrounds it some warm water, and you will find after a short time that there will be an increase in the volume of the liquid in the flask. The water will rise in the tube, springing from it—although the increase will take place very slowly. In the case of gases matters are very different, they expand very much more. But here is a vessel which contains air. It is a flask of about the same capacity as that which contains the inky water. It is connected at the lower part with a glass tube dipping again into inky water. The column of water placed in the tube by its height indicates the volume of air the vessel contains. But if I merely warm this vessel of air with my hands, I think you will see in a moment that the water in the tube descends. The volume of air increases rapidly, and the lowering of the water is an indication of the amount of this increase. So that you see gases expand very much.

An instrument of this kind is called an air thermometer, and I believe the first air thermometer ever made is in this exhibition. It is one of those relics one dare not touch; and I certainly could not ask the authorities to have it brought up here. It is in the case of Galileo's instruments, on the right-hand side, and consists of a small apparatus, with a little globe at the top,—a narrow glass tube dipping into the liquid below; and this, I believe, is one of the first forms of air thermometer ever made.

This same Regnault has experimented also on the expansion of gases by heat; and here is another of the relics, the original apparatus by which he studied the expansion of gases by the action of heat. It is more modern than Galileo's, and it will stand a little experimenting with. This is the vessel in which the expansion of gases was determined. It consists of a tube

blown into the form of a bulb with a long narrow stem bent at right angles and drawn out to a fine point. The first thing was to determine the capacity of it, to see how much air it held. This was done by filling it with mercury and weighing the quantity of mercury which it contained, because knowing the weight of a certain volume of mercury, if we know the weight of the mercury, we can easily calculate the volume. Having calculated the volume of the vessel, the mercury was emptied out, and the vessel was filled with air. The air had to be introduced with very great precaution. The air was passed through a vessel containing chloride of calcium, a body which absorbs moisture with great ease. The air was pumped out from this vessel through a chloride of calcium tube, and then was once more allowed to enter through a second chloride of calcium tube. In this way the air was dried and any trace of moisture which might have been on the sides of the vessel was removed. In order to hasten the removal of moisture the whole thing was placed in steam, so that the air was heated to the temperature of boiling water, the moisture was pumped out, and the dry air was allowed to return. This having been done several times, the connecting piece was taken off, and you had the apparatus standing in a bath of steam. When the temperature was perfectly constant a blow pipe was brought to bear on the point of the tube, and it was sealed up. Thus you had a quantity of gas included within it, and you had the exact volume of the vessel when it was heated to the temperature of boiling water. Now the question was to find out what volume would this gas occupy when it was cooled to the freezing point, and that was done in this actual apparatus. The tube is placed into this apparatus and held firmly by means of a screw above, and by three little supports by which it can be placed exactly vertically. When everything was perfectly in order, the point of the tube was broken off under mercury in a glass vessel below; and this point having been broken off, the mercury entered the tube and rose into the vessel. Then a glass vessel was placed around this, and a quantity of ice was put in it, so that the temperature was reduced to the freezing point of water. A condensation of the gas of course took place, and we had the column of mercury raised to a certain height. Now it was neces-

sary exactly to measure the height of this column of mercury by the cathetometer, but it was impossible to do this when it was surrounded by the mass of ice, and it was necessary to have recourse to what we call in the laboratory a dodge. A little piece of soft wax was carefully brought under the mercury and pressed against the end of the tube, and this being a fine capillary tube the wax stopped it up, and it was possible to remove the ice entirely, and to wipe the tube without altering the length of the column; the mercury could not escape because of the plug of wax at the end, so that it was quite easy to measure the height between the column of mercury outside, and the column of mercury within the tube. Then it was necessary to apply this law of Boyle of which I have spoken, to determine the amount of the expansion of the gas. I need not go into the calculations, but I will just point out the result of a number of experiments.

Regnault was not satisfied with one experiment of this kind, but he repeated it no less than fourteen times before he was satisfied he had the right number, and the number he got was this. If he took one volume of gas at the freezing point of water, and heated it up to 100 C., one volume increased to 1·36623. Perhaps some of you are not quite familiar with the meaning of these decimals which I am using, therefore, I may tell you that that means if you had 100,000 volumes of air at the freezing point of water, they would be changed into 136,623 if they were brought to the boiling point. Regnault was not satisfied with this, so he used instead of a large tube like this, a large bulb; and instead of having this short tube at the bottom, he used a thin tube, but a very long one, so that when the point of the tube was broken off, the mercury did not rise into the globe itself, but merely rose some distance into the tube. He tried a number of experiments of this kind, and obtained the number 1·36633 which is very nearly the same as the previous one, but not quite. This was not sufficient for him, and he continued further, and used an apparatus of which this is a model. This experiment was done in two ways. A volume of gas was introduced into a glass vessel, and this glass vessel was surrounded with ice, and its volume accurately determined. There is a globe containing gas,

placed exactly in the centre of this brass vessel, and underneath the globe is a kind of colander made of tinned copper to distribute the steam which had to be brought into it equally over the whole vessel. When the globe had been measured by introducing mercury and weighing the quantity, it was fixed into this apparatus, this colander placed on the top, and the whole thing packed with ice, and so the temperature was reduced to the freezing point of water. Then it was necessary to determine what change of volume would take place by heating the air. In order to determine this, the globe was connected with a long glass tube graduated accurately to millimetres and connected at the bottom to another graduated tube. Mercury was poured into the other tube, so that it filled both vessels exactly to the top of the closed tube. Then when the temperature was exactly at that of freezing water, and the mercury level was put exactly to the right point, a spirit lamp flame was put under the vessel containing the gas, so as to melt the ice, and ultimately to make the water boil. Of course there was an increase of volume of the gas in the globe, which, producing pressure on the mercury in this tube, tended to force the mercury down. But Regnault did not intend the volume to increase; he wanted to keep the volume constant. In order to accomplish that, it was necessary to very gradually pour mercury into the top of the open tube, so as to keep the volume of mercury perfectly constant at the upper portion of this closed tube. When the temperature was raised exactly to the boiling point of water, he measured the difference in height of the columns of mercury in the two tubes, and in that way determined the expansion of the gas. Of course he had to assume that the law of Boyle was correct, and the result had to be worked out from the increase of the pressure on the gas.

Now when this volume was kept constant and the calculation was made for the increase of pressure, he found that the number was 1·36645. I do not know how many experiments he made to determine this, but a considerable number. He was not satisfied, however, but he went a little further. Next he measured the actual increase of the volume when the pressure was kept constant, and for this purpose he

used an apparatus very similar, only the large tube was kept surrounded with water so that the temperature was kept constant. It was treated in the same way, and he then found a number which differed from all the others. The number he got when the pressure was constant, was 1·36706. He found different gases did not expand quite equally for different increases of temperature. These experiments are those taken with air, and I will put down beside these the effects produced by other gases both when the volume is kept constant and when pressure is kept constant.

	Constant volume.	Constant pressure.
Air	1·3665	1·3670
Hydrogen.	1·3667	1·3661
Carbonic Acid Gas. .	1·3688	1·3710
Sulphurous Anhydride	1·3845	1·3903

You see these numbers are not identical, but they do not differ very much from one another. Change of pressure changes the volume of different gases to very nearly the same extent although not absolutely, and change of temperature increases the volume of different gases nearly but not absolutely to the same extent. These last two gases which differ very considerably from air and hydrogen are gases easily liquefied; in fact this last one, sulphurous acid gas, can be liquefied very readily indeed. Here is a piece of apparatus which I always use to show the actual liquefication of sulphurous acid gas, except that I have a string passing over a pulley fixed to the ceiling by which I can raise the reservoir of mercury about 12 feet high. The sulphurous acid gas contained in one of these tubes is converted into liquid sulphurous anhydride, so that this has naturally a different expansive rate from the other gases. As a general rule we do not give the expansion of gases for 100° but for 1°, namely, from 0° to 1°, and the co-efficient of expansion of a gas is the alteration which one volume of it undergoes when the gas is heated from the freezing point of water to 1° C. Therefore the increase in all

these numbers would have to be divided by 100; and instead of giving to the first one ·3665, we should have ·003665, which is the co-efficient of expansion of air; that is to say, one volume of air would become 1·003665 if it were heated from the freezing point of water to 1°.

Now liquids instead of expanding as much, do so very much less. The increase of volume which water would undergo if it were heated about 1° at the ordinary temperature is about ·00015, in fact about $\frac{1}{20}$th of the expansion of air, and hence the small amount of rise in the column of water which we noticed in the experiment just now. In the case of alcohol it would be very different, its co-efficient is ·00108 so that alcohol expands seven times as much as water when it is heated 1°. You see, therefore, that if we take two different liquids we find their co-efficients of expansion differ very much, whereas if we take two different gases, we find the co-efficients vary very slightly, and the same kind of thing may be said with regard to the expansion of solids. They expand very much less than either gases or liquids. I will put down the co-efficient of expansion of iron which is ·0000355; that is to say, if you take 10,000,000 volumes of iron and heat them 1°, they would only become 10,000,355, whereas, of course, if you take a volume of air it would expand very much more. The co-efficient of the expansion of zinc is ·0000893, that is nearly three times as much as iron.

I fear I am wearying you very much with all this dry detail, but I want to point out one thing. I want to show you what scientific men have to do. Many of you are not perfectly familiar with the use of all the instruments in this collection. In fact I suppose no one is, but yet you will understand that something has to be done with all these things. These things here have been used only for the purpose of determining the expansion of gases; and the other instrument, which is in the other room, as being in the form in which it was first used, was only used for determining the compression of gases by pressure. You may say, what is the use of all this? At first sight it does not seem to be of much use. But I must tell you that all these

experiments of Regnault's were undertaken because the French government asked him to find out how it was the steam engine worked. He was asked to make an investigation of all the things which were connected with the working of the steam engine, and he set to work and thoroughly determined, and I believe with very considerable accuracy, the different effects of both pressure and temperature on almost all the gases he could work with, and a very large number of liquids. To come to a matter less important perhaps in one way than the steam engine, but nearly as important in another, no measurement of a gas can be made at all without first determining the effect of heat in expanding, or of pressure in diminishing the volume of this gas. If a mason has to measure a cubic yard, or a cubic foot of stone, he does not mind very much if he goes $\frac{1}{30}$th of an inch on one side or the other of the foot. It does not matter very much to him, therefore, whether the pressure of the atmosphere changes the volume of the stone, which it does, but very slightly, or whether any change of temperature has affected the stone, which it only does to a very slight extent; if one of you wishes to measure a pint of beer you do not ask whether the temperature is rather higher at one time than another, or whether you get a little more beer on one day than another, though I may tell you you get more alcohol if you measure it in the winter, for the volume is smaller, and therefore more concentrated. But when you have to measure a volume of gas to the $\frac{1}{1000}$th part of a cubic inch, then you must begin to think whether the temperature will produce any effect upon it or whether it will be at all affected by variation in the height of the barometer. A variation of one inch in the height of the barometer would make a difference of $\frac{1}{30}$th in the volume of your gas, and, therefore, it is necessary to know these numbers, and it is necessary for the numbers to have been determined before any gas analysis whatever could be made.

There were two or three other properties of gases which I wished to point out, but the time has gone, and it is impossible for me to do so; therefore, I must only repeat how extremely important these apparently dry scientific matters are. The work which scientific men have to do is, generally speaking, extremely

dry to those outside the laboratory, though extremely interesting to those in it, and very few can understand the manner in which we live. We are often working 10, 12, 14, or 16 hours a day, and we are looked upon as very monastic because we have not time to associate with other people and are always at work in our laboratories shut up like hermits. But sometimes a result comes out from this apparently dry work, and a great collection of instruments like this shows you in some degree the results obtained by men who have devoted the whole of their lives to these things, and have worked at them continuously, showing you that there is really something important in this dry investigation of natural science.

The CHAIRMAN: We have all listened with great interest and pleasure to Professor M'Leod's very clear lecture, and I am sure that you will all agree with me in passing him a vote of thanks for having given it to us.

ON LOCAL GEOLOGY, WITH SPECIAL REFERENCE TO THAT OF LEICESTERSHIRE.

By W. J. HARRISON ESQ., F.G.S.

August 14th, 1876.

MR. A. C. KING, F.S.A., IN THE CHAIR.

THE CHAIRMAN: Mr. Harrison, of the Town Museum, Leicester, will be kind enough to address us to-night on Local Geology, with special reference to the study of that of his own county.

MR. HARRISON: I wish to-night first of all to give you some idea how it comes about that we have such a thing as local geology at all; how it is that we have got so far as to have such an extended subdivision of the science, and then to try and point out what I think is the best way for any person who wishes to study the truths of geology to become acquainted with them, and also to show what work he may most usefully do; lastly, to illustrate this with some account of what we are trying to do inLeicestershire. In so doing I shall try as much as possible to make use of the collection exhibited in these galleries, because it is a very admirable one; there are many things which but for it we should not have had an opportunity of examining; and I think if these objects are alluded to and their special points brought out, they will be afterwards examined with greater interest.

The science of geology, the study of the crust of the earth, is of comparatively recent origin. This is mainly attributable to its dependence on other sciences and to the aid which it requires from them; and as unfortunately scientific work generally, with the exception of a little astronomy and a little chemistry, or perhaps one ought to say rather a little astrology and alchemy, dates from

since the Middle Ages, we can understand how it was that geology was not at all worked at or understood until very recently. Thus before we can geologically examine a district we require a good topographical map of that district, which is the result of a trigonometrical survey. Then in the study of the rocks, we require the aid of mineralogy, chemistry, microscopy; and for the determination of the contents of those rocks, the fossils, we want a considerable acquaintance with botany and zoology; and as all these sciences are also of comparatively recent origin, and as geology is so largely dependent on them, it is plain that that is the reason why it is of comparatively such a late date.

The history of the rise and progress of geological science in England has yet to be written, but I venture to say there are in it three stages—first, a time of speculation; secondly, a time of inquiry; and thirdly, a time of good and exact work. Now the time of speculation, I would say, began about the end of the 17th century. There have come down to us from about 1670 to 1700 the names of a group of men who investigated geology to some extent in England—Plot, Lister, Hooke, who was in advance of the rest, Ray, and Woodward, who bequeathed his collection to the Cambridge Museum, Burnet, Whiston and others. This effort in [illegible]17th century seems to have been the result of the inquiries [illegible] the controversy going on abroad, in Italy and elsewhere, [illegible]ut that time as to the nature of the fossils found in rocks. [illegible] a long time it was believed, as Dr. Plot in his History of [illegible]fordshire says, that shells found in rocks might be attributed to [illegible]plastic virtue latent in the earth. That was evidently an idea derived from Italian writers, who for some time insisted that these fossils in the rocks had never been really living things, but were just produced as accretions might be. When we read speculations of this kind, extravagant as they seem, we must remember that the investigation of facts had barely commenced. At this epoch in fact we are standing on the line of demarcation between the time when men evolved theories of the earth and the universe out of their inner consciousness, and the modern period which demands a thorough rigid and searching inquiry into the facts as the preliminary to all sound and true work.

We may now pass over a century, from the end of the 17th to the end of the 18th century, before we again find Englishmen taking a prominent part in geological inquiry. We now find we have to do with a very different set of men. A great deal of good progress has been made in other sciences, and this helps geology, and at the end of the 17th century we come to a knot of men of whom two were pre-eminent—Hutton, of Edinburgh, and William Smith, an English surveyor. These men laid the foundation of real geological work. Hutton was fortunate in his friends. There was at that time in Germany a famous mineralogist named Werner, who also wrote and spoke very well on the study of the rocks, and he too had a great group of friends, but there was a great battle between the followers of Werner and the followers of Hutton. The German insisted that all rocks were deposited from water. Even such a rock as basalt, which is to our eyes so thoroughly a volcanic rock, he would have was deposited at the bottom of the sea. But Hutton, on the other hand, took up the cause of volcanic action, and said a great deal was due to the action of volcanoes, and a very fierce battle went on between them. One of Hutton's friends was a gentleman named Playfair, and another who helped Hutton was Sir James Hall. Hall said if many of the rocks we see were really produced by heat, for instance granite, and if, as Hutton says, these rocks have sometimes altered others touching them—for everywhere we find rocks such as granite, syenite, and so on, have produced a curious change in the rocks close to them by their heat—any great hot mass of that kind would evidently burn, harden, and change anything it touched; and so Sir James Hall said if marble, for instance, were the result of the action of some great hot mass of rock on limestone, as Mr. Hutton says, then if we heat limestone sufficiently we ought to change it into marble, or something like it. He experimented in this way. First of all he heated some limestone in clay crucibles, but he got no result in that way. Then he heated other rocks in porcelain tubes, but in that way he got no result. Lastly he used large iron gun-barrels, and I have a fragment of one here, which was used by Sir James Hall about 1800, in which he submitted certain rocks to a white heat, and he

did succeed in effecting the change which we call metamorphism, proving that rocks such as marble, for instance, are really produced from other rocks, such as limestone.

William Smith was working in England while Hutton was working in Scotland, and this Collection contains some of the first maps drawn by William Smith. Here is one drawn by him about the year 1799 or 1800, and there are also some diagram sections drawn I think in 1799, and there is also his large geological map of England which was published in 1815. I must say that I was astonished when I examined these maps, at the progress he had made in the year 1815. That map is positively more accurate in one respect near the town of Leicester than the map of the Geological Survey, in one small respect which that survey omitted. It is an astounding thing to have been done in the infancy of the science, and by one man. William Smith is generally called the father of English geology; "Strata Smith" they called him at the time, because he thought and talked of nothing else but the arrangement of rocks in strata, or in beds, which he was the first to discover and prove. Thus our second period may be termed one of real inquiry into the facts. There were other men who helped, but these two stand chief; and I may take this second period as closing with the establishment of the Geological Society in London in 1807.

Lastly we have an epoch which we may call that of exact work; we have had that of speculation and of inquiry into the facts, and now we come to thorough and minute work. It was soon found that the proper mapping and examination of the strata even of a small country like England was a task lying beyond private or amateur bodies; and the *necessity* of the task was admitted by every one to be great. About 1835 the late Sir Henry De la Beche made a representation to the Government of the day, which ended in the establishment of the Geological Survey, of which he became the first Director. Under this able man, and his no less able successors, Murchison and Ramsay, the task of mapping the strata of the British Isles has proceeded on a scale which may fairly be described as one which still is unequalled. It has been the model of other countries. The Museum of Prac-

tical Geology and the School of Mines in Jermyn Street are outgrowths from it; and indeed, practically, we may say that its cost to the people of this country has been saved a hundred times over. At present a large number of able men are engaged in its surveys; and you see a part of the result of their labours in the admirable series of maps here exhibited. The Geological Survey is probably not as well known to the people of this country as it ought to be; but the maps which they exhibit here are certainly of the finest description. They take the Ordnance map, which is an ordinary map of the country, made with great accuracy by the Ordnance Survey, and then the people of the Geological Survey give to one of their men, trained to the work, so many square miles, and tell him to lay down on the Ordnance map the places where every kind of rock occupies the surface, and you see the result in such a map as this. Each of these colours marks the place where a certain kind of rock occupies the surface. The red is reddish marl; the yellow is clay, and the next is sand; then limestone; then clay appears again—and so on. Everywhere these geological surveyors walk over the ground until they have traced the order of the rocks and got them down on the map. That map is drawn to a scale of one inch to the mile; but they have also drawn maps on a scale of six inches to the mile; and here is a magnificent map exhibited by the Survey of Scotland, which is merely a part of the geological survey of Great Britain, on this scale. It is a map of the Ayrshire coal field, and is a stupendous work. In fact, nobody but those well up in geological inquiry can believe the labour necessary to produce such a map as this one. To the dwellers there—to the coal owners, and to every one engaged in the district, that map, I will venture to say, is of incalculable value. They not only draw maps of the surface, but they show what is beneath. If you were to make a deep trench across the country, you would get what we call a horizontal section—such a section as you see in a railway cutting; and they publish such sections of all maps that they draw. Then they publish deep or vertical sections of all the coal mines. Here is one; and every one of these columns represents some particular coal mine in the Lancashire coal field. In that way every coal field in England has been

drawn on a scale of forty feet to one inch. Every bit of coal is there represented by a black line, thicker or thinner, according to the thickness of the seam, while the shales and sandstones between are represented by finer lines. They also write memoirs descriptive of these maps, one being published with every map, describing the district it relates to.

With the establishment of the Geological Survey, geology became a profession; and the science has so spread that we may estimate the number of those who now take an active interest in the work at several thousands. We have first the Geological Society of London, the Geologists' Association of London, numerous geological societies in various parts of England—in Manchester, Liverpool, Edinburgh, Dublin, etc. The Science and Art Department yearly holds an examination in geology, at which several hundred certificates are annually obtained; and I can assure you that all these certificates demand and imply the possession of sound knowledge on the part of the recipients.

Continuing, then, a review of the subject at the present moment, I will classify the geologists of the day under three heads; for the state of the science is really the state of the men who are now working at it. First of all, we have what I may call theorists, generalisers, and specialists. These are what we may term the higher portion. Then we have professional geologists—the men of the Survey, those engaged in lecturing at various institutions, and those who make a living in various ways by geology. Thirdly, we have a great body of amateur geologists all over the kingdom, whom we may call the local geologists, and it is this last division we shall have to deal with. I propose this evening to try to point out to you how any person who really has a desire to study geology may best and most rapidly fit himself for that purpose; secondly, what work there is to be done; and then to illustrate these remarks by a brief description of the district with which I am best acquainted—the county of Leicester.

Now, in learning geology I need not say that if there is any person within your reach who is qualified to teach the subject, that you will learn more rapidly from him than if left to your own devices. The Science and Art Department has raised a

large number of teachers; and there were, in the year 1874 to 1875, 162 classes being conducted in geology, in connection with the Department; and attending these classes there were 3183 students. If you are within reach of a science class, taught by a good teacher, by all means join it; or attend a course of lectures on geology, such as those given by the lecturers sent out from Cambridge. But suppose you are far removed from them, and are not within reach of a teacher—or even if you are—then certainly your first step must be to master thoroughly some good elementary text-book. It is quite a mistake to commence with too big a book. It is much better to begin by thoroughly learning almost by heart a good first work, so as to fashion in your mind the skeleton of a system which you can afterwards fill up with details. Then obtain a geological map and memoir of the district in which you live; and, while reading your text-book, walk about the country as much as you can, keep your eyes open, and go over the ground again and again. Do not be disheartened if everything does not appear clear—as clear as the diagrams in your book; and do not expect to see fossils everywhere standing out of the rocks, begging you to take them. I find that invariably the case with my students. They go to places which I have, perhaps, described as containing a great number of fossils, and being of remarkable interest; and they come back and say they were unable to see one. That may be done over and over again. You have to get your geological eyes, and then you will find there is plenty to be seen. Take note of every small opening in the ground; measure it, and draw it, and take specimens of the rock; and always be careful to put at the time a label upon them; for, if that is not done, you are almost sure to make a mistake, or to forget where your specimen came from. In this way you will lay the foundation of a collection of rocks and fossils which may ultimately become of real service to science. Lastly, take care to see all sections of rocks, in railway cuttings, brick pits, quarries, wells in process of sinking, house foundations, drains, sewers, ditches, gravel pits, banks of brooks and rivers, and coast lines—if you should be fortunate enough to live in a country touching the sea. But you learn, as you study your

geology, to be thankful for small exposures of the rock. You will see much more in a mere drain cutting, as you progress, than you would at first have thought could possibly have been learned from it about a rock.

Books will not enable you to recognize minerals, rocks, and fossils, with the exception of a few of the more common kinds. You must have access to specimens. If you have a good local museum, go to it; or if you live in London, go to the British Museum, or the Geological Museum in Jermyn Street, and this will greatly help you; and if you write out lists of all the local specimens in the cases, endeavouring to make a rough sketch of them too, you will soon recognize the principal varieties. To be able to handle the specimens is a great point; and there are now so many cheap geological collections sold by dealers in fossils and minerals that it is well worth while to buy a cheap collection; and by handling the specimens over and over again to get to know every one by sight without having to look for the name. There are many such elementary collections of rocks and fossils exhibited here. Master every word that has been written on the district you live in. If you cannot obtain the original writings, you can probably borrow the books that contain them, and then copy them all out, word for word. I have always found that a very good plan for fixing it in the mind.

By this time, you will, I hope, have become a local geologist, and ready to take part in deciphering the portion of the book of nature which lies within your immediate ken.

Now, is there any use to which you can put yourselves? Is there anything you can do? I have often heard it remarked: "The Geological Survey has been over the district, and mapped all the rocks, and shown where they are; there is no work left for you to do. What good can you do by poking about after them?" Granting there was nothing to be done, it would be still essential for any one who wished to acquire a true knowledge of geology to go over the ground for himself. Unless you deny that geology has any educational value at all—and I believe it has the very highest educational value—it is certain that any one who wishes to be at all acquainted with geological science must study the

whole thing for himself. It is no use simply reading what others have learned, and told us about it; you must see everything for yourselves. But you may not be so fortunate as to have had the geological survey in your district, for the whole of England has not yet been done even on the one-inch scale. And supposing that to be the case, you may render great help to the science by learning all you can about the district, and when the surveyor comes to your place by communicating your information to him. In Leicestershire—the geological survey of the western part of which was done about 1860, by Professor Hull—there was a local geologist, living nearly at the centre—at Ashby-de-la-Zouch—the late Rev. W. H. Coleman; and when Professor Hull came down there in 1860, he found that the information that Mr. Coleman rendered him enabled him to do his work, not only more expeditiously, but far more efficiently than he could otherwise have done, a fact which he acknowledges in his memoir of the Leicestershire coal field.

The local worker should keep a record of his observations. Every fact, however unimportant, should be noted down. Many valuable facts have been lost or forgotten, for want of this precaution. All freshly-exposed sections of rocks should be carefully measured and noted at the time of their exposure. Railway cuttings, for instance, soon become smoothed down, and overgrown, and difficult of observation. A record of all sinkings and borings, etc., may also become of great service, and a person living on the spot has the opportunity of making these observations, and if not made at the time they become lost and forgotten. In collecting, endeavour to aid your local museum. This is a point which I speak feelingly upon, because I belong to a local museum myself. Take a specimen for yourself, and one for the museum. The local museum should aim at illustrating completely the particular district in which it is situated; and it cannot do this without the aid of local workers. Your collection cannot very well be too large. Do not be content with one specimen only of a thing; get a good many, and you may do more than you think by their aid. Here, for instance, is an ammonite (*A. catenatus*), which we find only in the Lower Lias

in Leicestershire ; and this ammonite exhibits very strange variations. It is, however, fairly constant in appearance, form, shape, and so on, and the alterations are slight; yet it is just possible, by collecting larger numbers, that we may be able to trace it passing into some other species of ammonite; and if we could do that, we should afford the very strongest help to the theory of evolution—to those facts which Darwin has spent all his life in finding out. This Collection contains the attempt of a continental geologist, Baron von Ettingshausen, to show that the fossil remains of one species of plant passes into another; and it is one of the most interesting things in the building. Here is a leaf of the tree *Castanea vesca*, or perhaps you will know it better as the Spanish chestnut. In many rocks the leaves of trees are preserved in great perfection. Here, for instance, from Dalmatia, and other parts of Austria, the gentleman I have spoken of commenced collecting a large number of leaves of trees and plants; and here is a leaf which is found in beds of what we call the *Eocene* age. Botanists examined this leaf, and gave it the name of *Castanea atavia*, while to the Spanish chestnut they have given the name of *Castanea vesca*. That implies that they both belong to the same genus; but that the species are different. This Austrian geologist, however, has got a great many other specimens which occur in rocks between this older *Eocene* period and the present time; and in his collection he traces these leaves through newer rocks, and endeavours to show that the one species gradually passes into the other. If we look at the two leaves together, any one would see that *C. atavia* is not the same as the Spanish chestnut; but in the intervening *Miocene* time there are leaves a little more like, and in the still later beds, known as the *Pliocene*, the leaves are a trifle more alike. And, lastly, we have the true Spanish chestnut growing now. So that it is something like the famous case of wheat at one end of the table, and a loaf of bread at the other. A person who had never heard of the thing before, when he saw the wheat at one end of the table and a loaf at the other, would not see any connection between the two. But when he was shown all the steps in the process of making bread, he would see the connection. So when we take these leaves, and see a gradual change, the forms

becoming nearer and nearer until at last we come to the Spanish chestnut, it is difficult to avoid coming to the conclusion that the plant we know now as Spanish chestnut is the lineal descendant of the tree which grew in *Eocene* times which has been said to belong to a different species. If we grant that even one species can change in that way, we shall do all that Mr. Darwin desires.

Not only has the Austrian geologist exhibited here some fine specimens, but a local geologist, Mr. J. S. Gardner, has exhibited some beautiful specimens of fossil leaves from Bournemouth. He has not been so bold as the Austrian, and endeavoured to trace the derivation of one species from another; but certainly his collection of leaf-remains of the Eocene period is one of the most interesting in the Collection. It is of exquisite beauty and variety. Mr. Gardner also exhibits a collection of shells and crustaceans from the Gault, a bed of thick clay at Folkestone. He shows about 400 species of crustaceans and shells from that bed. They are specimens of remarkable perfectness; and if every local geologist would work as he has done, for instance, we should very soon have the science of geology in a very different state from what it is at present.

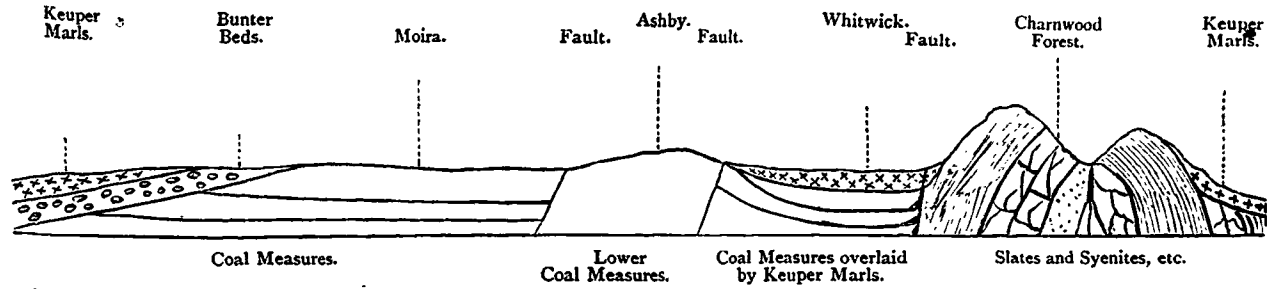
Having mastered thoroughly the structure of your district, you will be able to be of use to your townspeople, as to the best plans for drainage, for water supply, and possibly even in such a matter as the choice of a cemetery. In Leicester, for instance, some years ago, they laid out a beautiful new cemetery on the east-side, in a very stiff blue clay, which is admirably fitted to preserve the bodies of those laid therein. Well, if they had formed it on the west side, it would have been in sand and gravel, which would have been far better suited for the purpose. Encourage young workers and beginners by advice and aid; enlist all the quarrymen and miners in the study of geology, not only by rewarding them for any fossils they find, but by endeavouring to teach them something of the rocks among which they work. You will find here and there one who will repay your labour. Model your district, if you can. To the general public these Survey maps do not convey, at first, very much information; but if you can make a model of your district, such as this which was made by Mr. Clifton Ward, of the Geological Survey, and

represents part of the lake district around Keswick, it will make it more interesting. One of these models is coloured geologically and the other represents the glacial period, showing the direction of the flow of the glaciers most beautifully, and how they scooped out the basins which the lakes now occupy. There are many other ways in which good work can bedone—as in founding local scientific societies, reading papers, and so on; but it is already, I hope, plain that the local geologist can do much to benefit science generally, his district, and himself.

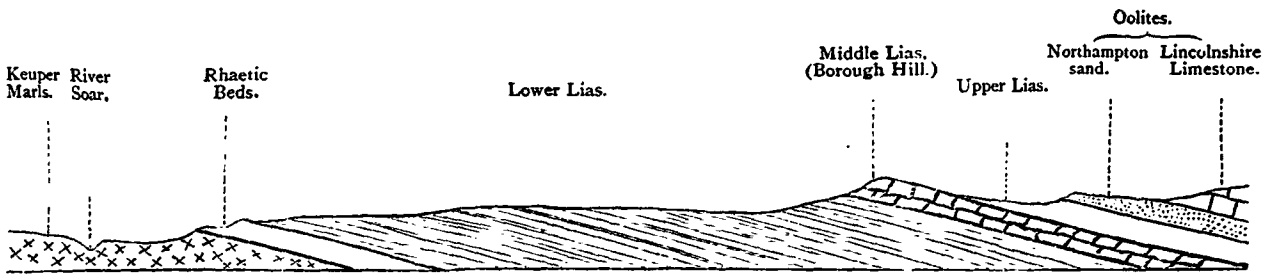
I will now briefly describe to you the geology of Leicestershire, because I can perhaps show in some degree the manner of work we are trying to do there. The county of Leicester is shown on this map, and there is also a diagram that you may imagine to represent a section of the rocks of the county. If you were to make two deep cuts, one across West Leicestershire and the other across East Leicestershire, so as to lay bare a great horizontal section of the earth, you would then see something like what is shown in these diagrams.

The unique feature of Leicestershire is the uprising in its north-western division of the mass of rocks which constitute the whole region of Charnwood Forest. This region of hard igneous and metamorphic rock seems out of place in the centre of England. I have often conducted over it scientific societies from other parts, and have always heard this exclamation, "How like Wales!" or "How like Cumberland!" To find such a region in the very centre of England seems almost to startle people. The Forest rocks rise to a height at the highest point of a little over 900 feet, and the region is composed of syenite, and at one place we find a true granite; there are also slates and volcanic agglomerates. You are perhaps more familiar with the rocks of Charnwood Forest in London than you know of, because a great deal of granite from the quarries there is sent up to London in the shape of thousands of tons of paving setts. Here for example is a specimen of Hornblendic granite, from the Mount Sorrel granite quarries. The stones of the street in fact afford a very good geological study, and especially after a good shower, which brings out their colours. There are two varieties of the Mount Sorrel

GEOLOGICAL SECTION.—WEST LEICESTERSHIRE.



EAST LEICESTERSHIRE.



granite, one of a whitish and the other of a pinkish tinge, due to the difference in one of the minerals, the felspar. At Mount Sorrel we find the rare mineral *molybdenite*, which is very much like lead, and marks paper like it, but it is a different substance. At Groby there are other quarries which yield a greenish syenite, and it was from there that Mr. Sorby, of Sheffield, obtained the rock which he sliced into thin sections, and endeavoured to prove the date at which it was formed. This making of thin rock sections is a modern branch of geological inquiry which is very useful, and there is here a case of Mr. Sorby's slices cut so thin as to be transparent. You can examine them under the microscope and see all the crystals they are composed of. In his memoir he describes the rock he examined as coming from Mount Sorrel, but it was really from Groby. These thin sections are also beautifully shown here by Mr. Clifton Ward. Of course you cannot see much of them, but here are some photographs which show the appearance of the rocks of the lake district, enlarged by photographic means, and you can clearly make out the different crystals which compose the rock. This microscopic geology lends great aid to the determination of the origin of rocks.

Charnwood Forest is our geological puzzle in Leicestershire, and it is a very hard nut to crack. There is an immense thickness of these rocks, slates and so on, but they have not yielded a single trace of life from beginning to end; they are put down on the Survey map as of the Cambrian age, simply because Professor Sedgwick, who first visited them, thought they belonged to the Cambrian period. That was in 1832. Professor Sedgwick had just come from Wales, and he put them down as being of the same age, but there is little evidence for it. They may be older, they may be of the Laurentian period, the oldest of all stratified rocks, and some geologists have lately considered that they belong to that period. For myself I prefer to bring them forward, and, instead of saying they were older than the Cambrian, to put them in the Silurian group. The reasons for that I could give you, but it would take too long to go into them.

There are some photographs here which we have had done of the principal exposures of rocks in Leicestershire, and if you look at them you will see they present many points of interest. The change in quarries and rocks is so great, that it is one thing which local geologists may do, to perpetuate these sections, to have them photographed at the time when they are well exposed. Unfortunately not only are there no fossils found in Charnwood Forest, but there are no other old rocks near to compare them with. The oldest rocks near to them are of the carboniferous age, and we know they must be a great deal newer, but unfortunately there are no very old rocks to compare them with. On the north of the Forest there is some mountain limestone and millstone grit, but no other rocks. Of the slates of Charnwood I have some specimens here, and they are curiously banded. To me they present the appearance of volcanic ashes, as if there had been going on during the time of their production a great deal of submarine ejectments from volcanoes, which were spread out on the sea bottom; and if that was the case we can easily understand that in those muddy disturbed heated seas there would be little chance for much life to exist. Moreover, this would aid us in determining dates. Neither the Laurentian nor Cambrian periods are marked by signs of much volcanic action, but the Silurian, which is more recent, is. By the bye, there is one place near Woodhouse Eaves where we have in the rock a number of concentric rings or furrows in the slate, but whether this appearance is of organic origin or not we cannot well say. There is nothing living now to which it can be referred, and it may be only an accretion.

There are some specimens also exhibited here by Mr. Sorby showing the cleavage of slates. You know that ordinary rocks which were laid down on the sea bottom will split along the planes of bedding. If you take a bed of limestone or a bed of marl or any ordinary stratified rock, it will split along the line of bedding. Slates, on the other hand, do not split along the line of bedding, and when you stand by the side of one of the great Charnwood slate quarries you can see what the original line of bedding was. First there is a streak of white, and then a streak

of red, and so on, but the rock does not split in that way, but perhaps at right angles to it.

The phenomenon of slaty cleavage was illustrated by Mr. Sorby, and has also been examined by Professor Tyndall. Mr. Sorby's experiments were most ingenious. He mixed clay with bits of blue blotting paper, and then subjected it to great presure in a hydraulic press; he also mixed clay with iron-filings, and treated it the same way, and these specimens were found to split at right angles to the line of pressure. Professor Tyndall has done the same thing, and has also improved the experiment in some respects by using wax. It was found that this slaty cleavage must have been the result of great pressure, probably side pressure. To anybody who is studying geology and how cleavage is produced in slates, the examining of the specimens exhibited here will be very useful.

But I must not stop at Charnwood Forest any longer. On the north of Charnwood there is *mountain limestone* and a little *millstone grit*, and then come the *coal measures*, which are marked on the map by thick black lines. The Leicestershire coal-field is often called the coal field of Ashby-de-la-Zouch, because that town is nearly in its centre; but the country about Ashby is nearly destitute of coal, although coal lies on each side of it. On one side there is the Whitwick coal-field and on the other the Moira coal-field, but at Ashby there is no coal. There we have a mass of rock like a great wall, composed chiefly of shales, etc., between the coal district of Whitwick and that of Moira. These beds ought properly to have been down below, and if we were to descend through the coal fields we should come upon the beds which near Ashby are at the surface. This mass of rock has been thrust up, and then all the upper part worn off. Probably the Leicestershire coal-field was continuous with that of Nottingham, but the Charnwood district being upheaved, the coal measures, with the beds of coal on the top, were denuded off, and now we get the coal beds running up against the metamorphic rocks of Charnwood. You see marked on the section also a very singular bed of rock marking the outflow of a volcanic rock, coming up between the coal measures and the Charnwood rocks; it then spread out on the top

of the coal measures, burning and baking the beds it flowed over, and in the Whitwick colliery they sunk through 60 feet in one pit and 80 feet in another of this basalt, which flowed out probably beneath the sea. Here are some specimens of it, and the clay it flowed over, burnt, baked, and charred. The proper name of this volcanic rock is *dolerite*. The central part is very compact, but the upper portion is much less so. The beds that rest upon it are not the least altered, so that it had time to cool before they were deposited. The Main coal which is worked is a good seam which is 14 feet thick in the Moira, and about 5 or 6 in the Whitwick division. About one million tons of coal are raised yearly from the Leicestershire coal field, and we calculate that there are at least 350 million tons waiting to be raised; so that it will only last 300 or 400 years, unless it should extend farther than we now know. It probably does extend eastwards underneath the rocks above.

Above the *Carboniferous* formation there ought to be the *Permian*, and there are one or two patches of rock of that age. Then we come to the great series of rocks called the *Trias*, because on the Continent there are three divisions of them. Here in England we have only two divisions—the lower, called the Bunter; and the upper, called the Keuper; the middle division, called Muschelkalk, you do not find in England. The bunter rests on the coal, but in one part the bunter is absent, and the keuper, or red marl, rests directly on the coal measures. That would show that this district was at a certain elevation at the time this bunter was deposited, and then the whole district went on sinking after the keuper beds were deposited. These Triassic beds, especially the keuper, are very interesting. One phenomenon they exhibit is what we call false bedding. There are many places in Leicestershire where the keuper sandstone shows lines ranged diagonally with little black particles of coal, making streaks; the great beds are laid one over another, but in each bed the line of bedding slants. We call that false bedding. It was for a long time a puzzle to account for it; but we now believe it was done by the action of currents of those seas drifting the sand over a submarine bank, where it settled. There is a model here, devised by Mr. Sorby, which illustrates that. Here

is a box which contains sand, and to represent the motion of the current there is a long tube with a screw carrying the sand, and as you turn the handle the sand is carried on just as a current of water would carry it. You see the sand is deposited in regular sloping lines, as was the case in these Triassic beds. That shows us that those seas were shallow, because we know that currents have no effect at a depth of 50 or 60 feet, or comparatively little effect.

There is another model, exhibited by Mr. Sorby, illustrating another point also. In many of these red marls you will find, what I dare say you have often seen at the sea-side, a number of wavy marks, which you call the ripple mark. You find these in old rocks millions of years old at least. The question is, how was that ripple mark produced? This model shows it. The white spots at the top are intended to represent the tops of the waves, and the lower ones the motion at the bottom of the waves, in passing over a sandy shore. When you turn the handle at the back, the upper portion will show the motion of the top of the wave, and the under portion will show the retarded motion at the bottom of the wave, which was caused by the sand. You will see that while the top of the wave moves in a beautiful undulation the bottom moves backwards and forwards, throwing the sand it would rest on into a number of curves, such as you see on any sea shore; so we have preserved to us in these ripple-marked triassic marls and sandstones, the record of that old sea beach.

In East Leicestershire we come to newer rocks. The town of Leicester is built on each side of the River Soar; near to it we get above the Triassic rocks a thin bed, which we call the *Rhaetics*. I was fortunate enough to be able to prove the presence of that formation in Leicestershire, for it was not known until two years ago that it occurred there. It was known to occur in the south-west of England and in Lincolnshire, and if it occurred there it seemed to me it ought to occur in Leicestershire also; so, hunting it up, and following the line where it ought to occur, I knew if it were present at all it would be between these red Triassic marls and these blue Liassic clays. But after long looking I found a section in it at last about 100 yards from my own house.

Just at the base of the *Rhaetic* beds there is a curious bed containing a mass of bones and teeth of several kinds of fishes, of ichthyosaurus, plesiosaurus, and fish spines. I found in that bed two new species of fossils, one a star-fish (*Ophiolepis Damesii*), and another a species of fish (*Pholidophorus Mottiana*). Above the Rhaetic beds, which are of no great thickness, we get *liassic* clays about 500 feet thick, forming the great pasture lands of East Leicestershire, which is a famous hunting district, and is nearly all in grass. Cattle fatten there readily, although the clays only grow long grasses and rushes, and it is there that most of the Stilton cheese is produced.

Then you come to a long slope of the *middle lias* capped by a red band called *marlstone*, which contains numerous fossil shells mostly of one or two kinds (*Rhynconella tetrahedra*, and *Terebratula punctata* occur in masses). Then come the *upper lias* clays, and in the extreme east of the county the *Northampton sand*, which they are working very largely in Northamptonshire for iron. It is not so rich in iron in Leicestershire. Above the Northampton sand we find the *Lincolnshire oolite limestone.* A gentleman who did a great deal in arranging this Collection, Prof. Judd, was the person who, in surveying East Leicestershire, ranged these Oolitic beds in their proper place. They were formerly placed with the Great Oolites of the west, but they really belong to the Inferior Oolites. Mr. Judd's map of Leicestershire, with the memoir he wrote to accompany it, is a piece of the most admirable geological work I ever had the pleasure of examining.

That concludes the series of rocks which make up the solid mass of Leicestershire; but there are also, scattered over the surface, irregular beds of gravel, sand and clay, which we call the *drift.* Now the Geological Survey in this country does not show the drift, but really it is to farmers and agriculturists a most important part. They do not care so much about what makes the solid crust of the earth, as what makes the soil itself, and it is the *drift* which makes the soil. In the map of London on a six inch scale they have tried to show the drift—where the sand occupied the surface, where the gravel, and so on; and I believe on all their maps in future they are going to show the drift, or to have special maps for showing it.

Then there are the river sands and river gravels: you must not think they are entirely barren, for they give rise to a most interesting geological inquiry, where geology and archæology meet, namely the question as to the origin of man. You know there was a time when men used tools of stone, before they found out metals. The first tools they used were very rough, merely lumps of flint chipped out roughly. We get no relics of that period (which is called the *palæolithic* age) in Leicestershire, but we do find relics of the *neolithic* period, when they not only shaped their tools roughly but rubbed and polished them. Here is a skin scraper made of flint, which I found only a little time ago, which is interesting because a single instrument forms a skin scraper at one end, and at the other end it is hollowed out to form an arrow scraper, to give the round form to the arrow stems. Here is a curious article which I brought up with me, to see if any one can tell me what it was used for. It is a hollow ring of stone; it may be a weight for a digging stick. The savages in Mexico use a stone something like this to weight their sticks. An ordinary stick will not make much impression on the ground, but if you weight the end of it you will be able to force in the stick much better. The people of the Solomon Islands use something similar for the heads of their clubs, and this may have been used for either of those purposes. Here is another specimen known as a *lucky stone*, a pebble with a hole through it. This was preserved for many generations in one family, until within the last few years, and great virtues were attributed to it. There is a label attached to it which says that it prevented the entrance of fairies into the dairy, kept off agues, and charmed off warts. It is an interesting relic. Apparently long after the stone period, when people had forgotten all about the use of these implements of stone, they venerated them when they found them. In Ireland, for instance, these flint arrow heads are known as fairy arrows, and in the British Museum there is a beautiful gold Etruscan necklace which has a flint arrow-head as a pendant. It is possible that our lucky stone may be a relic of the superstition of that time,—the belief in the sacred character of such stone objects.

In conclusion, let me express a hope that in the Science

Museum, which I trust the admirable collection in these galleries will lead to, the claims of geology will not be forgotten. Illustrations of the history of the science, together with educational apparatus of all kinds to aid in its study, may there find a fitting home, and fill a gap which is not filled by either of our great National Institutions. Such a collection would, I am strongly convinced, tend to popularise the study of geology in this country, which has been the object towards which my observations have been directed.

The CHAIRMAN: You will allow me, I am sure, to return thanks on your behalf to Mr. Harrison for this very interesting lecture. It will no doubt have the effect of inspiring many of us with the desire to pursue the study he has brought before us so pleasantly.

ON THE PHYSICAL GEOLOGY OF IRELAND AS COMPARED WITH THAT OF GREAT BRITAIN.

By Professor E. Hull, F.R.S.

August 15th, 1876.

Major Donnelly, R.E., in the Chair.

The Chairman: Ladies and Gentlemen,—It is my duty this evening to introduce to you Professor Hull, Director of the Geological Survey of Ireland, who will now give you a lecture on the comparative physical geology of Ireland and Great Britain.

Professor Hull: Ladies and Gentlemen,—You will be aware that in the short space of an hour it would be impossible for me to give any very lengthened account of either the analogies or the differences between the physical features of Ireland and those of Great Britain. I have, therefore, selected four subjects, each of which I hope, with your patient attention and my best efforts, I shall be able to explain to you.

I shall take, in the first instance, the different mountain ranges of Ireland and endeavour to show what are their respective ages, geologically; and also what mountain ranges, or groups, in Great Britain belong to the same geological period. Next, I shall take the question of the formation of the central plain of Ireland; thirdly, I shall endeavour to answer the question, which has frequently been put to me, "why is it there is so little coal in Ireland as compared with Great Britain?" and, fourthly, I shall give you some account of the origin of the numerous lakes which are scattered over the greater part of the central plain.

Now, Ireland may be considered as a great cauldron; a great

plain bounded in various directions by mountains, either as ranges or as groups; but not entirely surrounded by mountains; as, for instance, from the coast between Dublin Bay and Dundalk Bay on the east, to Galway Bay on the west; and if we took such a line, we should find that it passed almost entirely over that great blue formation on the geological map known as the Carboniferous Limestone. But in other directions we have different mountain ranges, and dividing them into five groups we shall consider them in this order:

1. The North-Western Highlands of Donegal. 2. The Western Highlands of Mayo and Galway (Connemara). 3. The South-Western Highlands of Kerry, Cork, and Tipperary. 4. The South-Eastern Highlands of Wicklow and Dublin. 5. The North-Eastern Highlands of Louth and Down (Carlingford and Mourne).

Now, what is the geological age of these different mountain ranges? Do they belong to different geological periods or to one? We are fortunately in a position to answer these questions with considerable accuracy as regards this country, for our evidence is very definite. Taking first the North-Western Highlands of Donegal, we may consider them in fact, as continued into Mayo and Galway, separated by this great Donegal Bay, but physically forming one range of mountains. This range is also simply a continuation, rising out of the sea, of the North-Western Highlands of Scotland; the strata belong to the same great group of rocks, the great "metamorphic group"; and they have been first upheaved from beneath the sea, and then transformed from their original condition into that class of rocks which we call "metamorphic" at some distant geological period.

Now we come to consider what this geological period was. But first I had better state to you what we understand by the term "metamorphic rocks." If you look at this geological map, which I may state is the first geological map of Ireland which ever was constructed, and made by him whom we call "the father of Irish geology," as William Smith was "the father of English geology," I mean Sir Richard Griffith — it is a beautiful specimen both of workmanship and industrious labour carried out over a

series of years, of great acumen and judgment in collecting, unravelling, and correlating the geological evidence—referring then to this map of Griffith's you will see that the Donegal mountains are partly composed of strata which are coloured red, others which are coloured purple, and others coloured yellow. Now the red means foliated granite, the purple means schists of various kinds, the yellow means quartzite, and these rocks are called "metamorphic" because they do not exist in their original state, but have been metamorphosed, or altered in their condition, by what we may call aquithermal processes—that is to say, by heat, and highly heated water under great pressure. I will not enter into that question now, but merely state that the original slates of which these mountains were composed have been converted by this metamorphic process into mica schists and hornblende schist, and a variety of other rocks, and sometimes a step further into gneiss, and ultimately, very often, into granite; that the original sandstones of which these beds were composed have been converted by the metamorphic process into quartzite, coloured yellow on the map; that limestones have been converted into crystalline marbles, and that some of the pyroxenic rocks, the trap rocks of the period, have been converted into hornblende rock and serpentine.

Now the period at which this alteration took place was the period to which we may refer the origin of these mountain ranges. It was the period in which not only the metamorphism took place, but these rocks were crumpled and foliated and thrown into great flexures ranging, as the map almost itself suggests, in north-east and south-west directions. If we refer to the geological map of the British Highlands, we find that this direction exactly coincides with the trend, or range, of these great masses of crystalline rocks of which the Highlands of Scotland are composed, so that these are physically referable to the same period of time; while of similar kinds of rocks are the Highlands of Mayo and Galway, including that beautiful group of conical hills formed of quartzite;—the Twelve Bins of Connemara. And here it is that we are able to determine the geological age of this range of mountains—that is to say, the period when they were first

elevated above the sea and transformed by this process of metamorphism; because on both sides of Killary Harbour, that remarkable fiord, the best example that occurs, perhaps, in Ireland of the Norwegian fiord—on both sides of that deep arm of the sea we find strata which are not metamorphosed resting in a discordant manner on these metamorphic rocks of older date. Now, what is the age of these non-metamorphic rocks — rocks which are in their natural condition, which are full of fossils, and therefore have not shared in the changes which the old rocks have been subjected to? The evidence is quite conclusive that these newer rocks are a portion of the Upper Silurian, ranging from the Upper Llandovery beds down to the Wenlock and Ludlow. From a table of strata you will be able to judge exactly the position of these two groups of rocks. These metamorphic rocks of which we have been speaking, of which these mountains are formed, consisted originally of strata belonging to the Lower Silurian system, and perhaps to part of the Cambrian, ranging through these series of beds up to the top of the "Caradoc or Bala beds." I do not enter into the evidence of that. You must please to take my word for it that it is quite conclusive. That the metamorphic rocks of Connemara are in reality Lower Silurian strata is the view expressed by Murchison, Harkness, and others, and confirmed by the investigations of the Government Geological Surveyors; on the other hand, the fossiliferous strata which, on both sides of Killary Harbour, rest unconformably on the metamorphic rocks, are known by their fossils (which are numerous) to be of Upper Silurian age. The strata which rest discordantly upon them range through the Upper Llandovery up through the Wenlock and Ludlow beds of the Upper Silurian, and, therefore, these great crumplings and foldings, and metamorphisms have taken place at a period represented by no formation at all; but in the interval between the Lower and the Upper Silurian periods. Therefore, we refer this range of mountains to the close of the Lower Silurian period. We refer the Donegal range to the same period, and we refer the mountain ranges of the Highlands of Scotland to the same period also. Now, that

is a period of vast antiquity, and I hope, if you ever visit these beautiful ranges of mountains, which I strongly recommend you to do, that you will look upon them all with feelings of veneration, considering their vast antiquity. On the whole of the continent of Europe except Norway there is scarcely a range of mountains of such antiquity as these North-West Highlands of Ireland and Scotland. The Alps were then slumbering in the womb of futurity, and it was not till long after that they began to raise their heads above the waters of the ocean.

Now we have to deal with the age of another beautiful mountain range extending from Dublin Bay southwards through Wicklow and Wexford. I need not expatiate on the charms of the scenery of Wicklow: it is known wherever the English language is spoken. You will see by the map that this also is composed to a great extent of granite rocks (coloured red) and also of Silurian rocks of similar age to those of which the mountains of Galway and Donegal are composed. But it is only to a small extent that these rocks have been metamorphosed; only, indeed, in close proximity to the outbursts of the granite. Now we have no direct evidence of the age of the Wicklow Mountains further than this, that it is older than the Old Red Sandstone, because we find the Old Red Sandstone to the north of the borders of the counties of Waterford and Carlow coming in contact and resting upon these granitic rocks. We know, therefore, that the Wicklow Mountains are older than the Old Red Sandstone, but I think we have also evidence that they are of the same age as the Highlands of Donegal and Mayo, because you will perceive that the range, or trend, of this granite is almost exactly parallel to that of the rocks of the Donegal Highlands; they both range in a north-easterly and south-westerly direction, and geologists know that parallelism of direction is strong evidence of synchronism; that when we find two ranges of mountains parallel to each other within moderate limits, the probabilities are that they have been elevated into mountain ranges at the same period of geological time. Therefore on that ground I maintain that the Wicklow and Dublin mountains date as far back as the end of the Lower Silurian period.

Now we have to deal with another range of mountains, that perhaps which is most generally known—the beautiful, grand, sublime mountains of Killarney, rising to an elevation of 3,414 feet in Carrantuohill, one of the peaks to the south of the lakes of Killarney; a name which is of Celtic origin, and which, curiously enough, speaks of the remarkable perception and remarkable powers of description of those who gave it the name. Carrantuohill means literally an inverted reaping-hook; and when this mountain is looked at in certain direction, I believe from the southward, the side of it presents exactly that appearance. You will observe, on looking to the geological map, that the direction of these ranges of hills, which are very clearly shown by these headlands running out into the Atlantic, is very different from that of the Donegal Highlands. Instead of being N. E. and S. E., the direction is very nearly East and West. These lower bands of green represent the Lower Carboniferous rocks, and the yellow and brown show clearly the direction of these ranges of hills. Now, these rocks are also much newer in age than those of which we have been speaking; they, in fact, belong to the Old Red Sandstone and Carboniferous periods, so that these mountains of Kerry and Cork are very much more modern than the mountains of Mayo and Donegal, and are composed of more recent strata. This range of mountains consists of a great number of flexures. The whole of the strata have been at one period crumpled up (if I may use the expression), laterally crushed by the contraction of that part of the earth's crust, so that they are thrown into a series of great flexures, sometimes to such an extent that they have been doubled over on themselves. These mountains being composed of the Old Red Sandstone and Carboniferous formations, are of course newer than the Carboniferous period, and the question arises to what geological period are they to be referred as a group of mountains? Now, in the S. W. of Ireland we have no direct evidence on this point, because these Carboniferous and Devonian rocks nowhere come in contact with any strata of a newer age. They do not come into contact with any New Red Sandstone, or Lias, or any of these comparatively modern rocks; so that really, as far as Ireland itself is concerned, we have no guide as to the age of these

mountains. But when we come across the Channel, and observe the position, and character, and arrangement, of the formations in the S. W. of England and in South Wales, we find that in Devonshire and Somersetshire the strata belonging to the same geological age are thrown into precisely similar flexures to those in the S. W. of Ireland; in fact, the one set of flexures here are merely the continuation of the other, and they belong to the great system which has extended through the North of Europe, through Belgium and France, and away beyond the Rhine. In the S. W. of England and South Wales we have very direct evidence as to the age of these flexures; for there we find the Triassic (or New Red Sandstone) strata resting unconformably upon these older rocks, which have been thus tilted and upturned; and therefore we feel sure that these flexures were produced before the period of the New Red Sandstone. But we can go a step further, and from analogy and reasoning, which I cannot enter into now, I have no doubt that these flexures are of still older date, and that they were produced before the Permian period; that they were produced, in fact, immediately upon the close of the Carboniferous period itself. So much, then, for the age of the second group of mountains.

Then we have to deal with the North Eastern Highlands of Mourne and Carlingford—another very grand and beautiful range of mountains, perhaps less known than any of the others, rising in Slieve Donard to an elevation of about 2,796 feet, a conical mountain rising almost directly from the sea. This group of mountains is composed of a great variety of rocks, various kinds of igneous rocks, and surrounded by Lower Silurian slate rocks. But we have no evidence to tell us what is their age, further than that they are newer than the Carboniferous period, because they have been intruded among the Carboniferous rocks. But as no formation newer than the Carboniferous occurs in the district, we have no evidence regarding their absolute geological age. They are, however, remarkable and unique. The character of the granite of that district shows numerous volcanic phenomena, closely resembling some portions of the Highlands of Scotland, namely, the mountains of Arran. So that I am strongly

impressed with the conviction that, whatever may be the age of the granite of Arran, which some people think as new as the Tertiary, such is the age of the granite of Mourne. I cannot concur in the view that the granite of either Mourne or Arran is so recent as the Tertiary Epoch, as the volcanic rocks of this date (viz., the Trachyte porphyries of Antrim and Down) are very different in character from the rocks of Mourne and Carlingford. On the other hand it is quite possible these latter may be as old as the Permian period.

Now, I come to the second part of my subject, and this involves the third part; so I shall treat them both together—namely, the formation of the central plain of Ireland, and the cause of the small amount of coal that is to be found over that part of the country as compared with England. I have already said that the centre of Ireland is, in fact, a great plain, bounded in various directions by mountains, and towards the south containing several grand isolated eminences, such as that of Galtymore, which rises to the height of 3015 feet above the sea, and is a very grand object when seen from the northward. How was this great plain formed? It seldom rises more than 300 feet above the level of the sea, perhaps 250 feet is about the average; and it is covered by lakes, and by bogs which were once lakes, and to a large extent by gravels of very recent origin. The foundation rock of the central plain of Ireland, as you see by referring to the maps, is Carboniferous Limestone; and whenever we find Carboniferous Limestone, we infer, as geologists, that the coal-formation once existed. It is the foundation, in fact, of the coal-formation; and just as a traveller in exploring some country like Egypt or Assyria, when he comes upon a ruined foundation of some building, feels sure that the superstructure once existed, so when we find Carboniferous Limestone we know, of a certainty, that the coal-formation once existed also. If your look over the geological map, you will see that an enormous extent of country must have been covered by the great coal-formation. It was the whole of that coloured blue, the whole of this brown colour (which are more recent beds), and the whole of that coloured yellow (Old Red Sandstone)—the whole

of that region was covered at one time, at the close of the Carboniferous period, by a continuous coal-formation. Of this, however, we have only a few relics here and there; and the question arises;—Why is it that coal is so largely contained in the strata of Great Britain, and to so so small an extent over this Carboniferous area in the centre of Ireland? When you look at the geological map of England, you will find the coal-fields coloured very dark, and they pass underneath other strata coloured red, of various shades, and towards the south, brown, yellow, and green. In fact the coal-formation of England is to a very large extent concealed, and protected beyond its margin by newer formations, as the Permian, the New Red Sandstone, or Trias, Oolite, Chalk, and the Tertiaries. We do not know to what extent the coal-fields extend under these newer formations, but we know that they do so to a large extent. Now, at the close of the Carboniferons period in Britain very nearly the whole of the coal-fields were thus concealed, and covered up by these newer strata. The country began to subside vertically under the waters, first of the great lakes, until the New Red Sandstone was deposited, and part of the Permian beds; and then under the waters of the sea, while the Lias and Oolite rocks were deposited; and then still farther under the waters of the sea, until the Cretaceous rocks were deposited; so that they were ultimately covered over by hundreds or thousands of feet of these newer formations. Thus you see what a great protection these newer formations have been to the underlying coal; and in process of time the whole of these rocks have been lifted into dry land, great masses have been swept away, and the underlying coal-fields have been revealed, and brought within our reach.

But in Ireland it seems to have been otherwise. Except in the N. E., in the counties of Antrim and Down, we have scarcely any representation of these newer forms. The whole of the centre, the South and West of Ireland, is completely devoid of any strata newer than the Carboniferous; and therefore it is clear that a very different set of conditions prevailed over the area of Ireland than over the area of Great Britain and the Continent. It may be asked why these formations are absent from the Irish area? It might be supposed that they had once been deposited over the

Irish area, but had subsequently been swept away by denudation. But I cannot accept that view. I feel satisfied that if these newer formations had once been there some vestiges of them would have been left in the protected places among the older rocks. But we have no trace of them whatsoever. A small patch of Triassic beds occurs at Carrickmacross in Co. Louth, and may be considered as the southernmost limit to which these strata once extended. My theory to account for their absence is, that while the British area was being submerged beneath the waters of the sea, and was receiving great marine deposits, the Irish area remained in a state of dry land, not submerged nearly throughout the whole of this period; or if at some time slightly submerged, it never was to any great extent, and, therefore, it did not receive these newer deposits over its Carboniferous rocks.

The consequence of this was that while the British coal-fields were being protected by these great deposits of newer formations to the thickness of hundreds or thousands of feet, the coal-fields of Ireland were exposed to the agents of denudation, to the action of the sea, of rains and rivers; and their operation, gradually, little by little, extending through the lapse of the Mesozoic period, was sufficient to carry away nearly the whole of the coal-formation of that part of the United Kingdom. Thus it is that the character and habits of the Irish people were determined ages before the landing of Strongbow, or any other great event in the history of Ireland. The destruction of her coal-fields has caused agriculture to be the unavoidable occupation of her people; and thus it comes to pass that while we have in Britain an industrious and very go-a-head race of manufacturers and operatives, we have very few of this character in Ireland, except in the north and eastern portions, which form an honourable exception. The coal-fields then, I believe, were denuded away; and we have only remaining these little scraps, I may call them, in Tipperary, Kilkenny, and in the North of Ireland. These only remain as slight memorials of the great formation which once covered the whole of the country. Therefore, I think I have given a very sufficient answer to the questions, Is there any coal in Ireland?

or, Is there much coal in Ireland? and I hope a satisfactory reason why there is so little.

Now I come to the fourth and last subject of analogy and contrast; and this is the subject of the contrast between the physical features of Ireland and those of Great Britain. No one who has travelled over the great central plain of Ireland, particularly the northern parts of West Meath and Cavan, and the upper reaches of Loch Erne and the N.W. districts, can fail to have been struck with the immense number of lakes of all sizes, some of them pretty, I will not say beautiful, and some of them beautiful. When you get to the N. W., the district of Sligo, you find some beautiful lakes indeed, surrounded by bold escarpments and beautiful wooded banks. In nearly every district where these lakes are situated it will be observed, on looking at the geological map, there is the same blue formation of Carboniferous limestone. In addition, if you examine the coast throughout the Western and Southern and Eastern districts, you will find that wherever the blue formation goes down to the sea, there we have a deep indentation of the sea itself, forming deep bays. We have Sligo Bay eaten in amongst the blue rocks. Then we have Killala Bay; Clew Bay, which has eaten so far into the coast that very little blue rock remains; and then you have the great Bay of Galway; then come the beautiful harbours of Cork and Waterford, Dublin Bay, and Dundalk Bay; and all these bays are more or less eaten in, as it were, by the sea, wherever the Carboniferous limestone forms the surface. Now this number of bays, and the number of lakes in the interior, are closely connected with each other, because the one is due exactly to the same cause as the other. And what is the cause of these numerous lakes? In England you have no such lakes at all. You have the Carboniferous limestone present very largely in some parts of the country—for instance, among the Derbyshire Hills; and you have other limestone formations. In the central counties they are very largely developed, but you have no lakes amongst them, as is the case in Ireland. The cause is twofold. In the first place, it is the solvent power of water when charged with

carbonic acid, and spread over limestone. Whenever water contains a sensible quantity of carbonic acid it immediately acts upon limestone, and like an acid begins to dissolve and eat through the rock, however hard and solid it may be. And in the second place, it is the comparatively low elevation of the central plain of Ireland above the sea. This naturally involves very sluggish streams. The rivers which take their rise in the interior, and meander through these plains—such as the great river Shannon itself—have a very slight fall, and are therefore exceedingly sluggish. But they are all more or less charged with carbonic acid taken up from the decaying vegetable matter; and as they wander among these limestone rocks they continually act upon them, gradually dissolving them away; and by their sluggishness, almost stagnant in places, they have the power of spreading out laterally, and ultimately form chains of lakes. In connection with these there are also numerous underground rivers in Ireland. We know there are such in Derbyshire, in similar places, among the mountain limestone hills of Derbyshire; and they are also found in other parts of the world—in Syria, and in the great Cave of Kentucky. One of the most remarkable of these underground river-courses that I know of—in fact, the largest underground river, I suppose, in Britain—is that which conveys the waters of Loch Mask into Loch Corrib. The whole of the waters of the northern lake, which is at the west side of the great limestone plain, pass underground through a concealed channel in the limestone rock into the head of lake Corrib, and ultimately into Galway Bay. When some of these rivers have been going on for some time tunnelling through the limestone, the roof frequently gives way, and the commencement of a lake is made; and then the waters, still eating away the rock laterally, ultimately produce a lake. Therefore you see it is owing to the solvent power of the water, and then to the low level at which the rivers run, which prevents them having a rapid fall, that we have these numerous lakes. We may call them "lakes of solution;" and at a former period, just at the close of the Post-pleiocene, or Glacial period, they must have been vastly more numerous than they are at present; because almost all

those great bogs, such as the "bog of Allen," of which you have heard, which really means no particular bog at all, but a great chain of bogs in the interior, have at one time been lakes; and for the most part they are underlaid by beds of calcareous marl, which are neither more nor less than decomposed masses of fresh-water shells, so that before the bogs were formed the lakes of Central Ireland must have been exceedingly numerous. I should also add, that the beautiful lakes of Killarney have had their origin in causes similar to those above described.

As a point of contrast we may say that we have no such lakes in England; because for the most part these limestone rocks are all elevated into the high districts, and consequently the water having a rapid downfall has not the power of spreading out and forming lakes, as in Ireland. Some one will probably reply:—But what are the Cheshire meres? The Cheshire meres are the only partial representatives of the lakes of the centre of Ireland; but they are due to an entirely different cause. In the first place, the meres of Cheshire do not overlie the Carboniferous limestone at all, but they overlie the Keuper marls, which contain beds of rock salt. Mr. Ormerod has suggested what is generally accepted by geologists as the true solution of the question—that the rock salt has locally been dissolved away, and the ground surface has sunk, and then the surface waters were poured in and formed these little meres. But you will see that although they also are lakes of solution, they are entirely different to those I have been describing.

Now, lastly, as a point of analogy, and not to leave the subject entirely untouched, we have some great lakes in the mountainous districts of Ireland, as well as in the mountainous districts of Britain; and these are strongly analogous to each other. They are referable, I hold, to a great extent, to glacial erosion. That is the view of my distinguished chief, Professor Ramsay, as to the origin of these lakes. I have now taken up as much of your time as I am entitled to do; and I can only thank you for your kind attention.

THE CHAIRMAN: I am sure you will all join in a most cordial vote of thanks to Professor Hull for his extremely interesting lecture.

THE ANALOGY BETWEEN LIGHT AND SOUND.

By Professor Barrett.

August 19th, 1876.

Major Festing in the Chair.

The Chairman: Ladies and Gentlemen,—I have to introduce to you Professor Barrett, of the Royal College of Science for Ireland, who is going to give us a lecture with illustrations showing the analogy between light and sound. I think I need not do anything more than introduce him to you.

Professor Barrett: Ladies and Gentlemen,—It will be my endeavour in the present lecture, that I have had the honour of being asked to deliver here, to bring before you a series of experiments showing the relationship that exists between sound and light. At the outset permit me to remind you there is no direct bond of union between sound and light; they cannot be converted into each other, as many of the other physical forces can, but there is a similarity in their manifestation, a parallelism in their phenomena, which is exceedingly helpful in the study of the laws common to both. The analogy of sound and light has, it is true, long been noticed, for even in *language* we find an interesting recognition of this analogy. The same words are frequently applied to sound and light. We often speak of a *loud* colour as well as a loud note,

and every one, I presume, is familiar with Locke's blind man who called the sound of a trumpet, scarlet; and a friend, whom I see before me, has known of a case in which a person suffering from illness and blindness always spoke of a sequence of notes as if they were a sequence of colours. The words Dim and Dumb are, I believe, cognate terms in Anglo-Saxon. To speak and to shine, though often very different things in the lecture theatre, are yet denoted by the same words in Greek and in Sanskrit. (See *Appendix* A.) We might trace this correspondence much further, but our business is with the scientific aspect of the question. Upon that let us enter. In order to make this relationship clear, we must travel over ground that will no doubt be familiar to some in this audience, and hence I must ask such to bear with me occasionally for the sake of others.

Almost every one is aware that sound is due to vibration; light also springs from the same cause. Only in the case of sound it is a vibration of sensible masses of matter, in the case of light of quite insensible molecules. This, indeed, is true throughout the analogy; sound is a gross representation of its sister light. Here I have a tuning fork, upon drawing a bow over the prongs of which you will hear a sound. The fact that the fork is vibrating can be easily demonstrated by bringing it against a little light suspended ball. The moment it touches the ball, the latter will be tossed to and fro, and as the sound of the fork dies away the ball will gradually come to rest. Vibrations of molecules giving rise to light cannot be made evident to the eye, the smallness of these molecules and the rate of their motion transcend the grasp even of our highest instrumental powers, and can only be deduced by reasoning which is rendered easier by this very analogy we are considering.

Let us now enquire in what way these vibrations reach the eye and ear respectively. Imagine a stick plunged in a lake of still water and moved to and fro; a series of waves will be produced travelling outward and onward from the centre of disturbance. These ripples moving onwards will finally strike the weeds which may grow on the bank, and bend them to and fro in perfect synchronism with the motion of the stick in the centre of the

lake. Just in the same way does the sound of my voice excite aerial pulses through the air, and bends the drum of your ears to and fro in perfect accord with the vibrations which are taking place within my larynx. We can obtain an idea of this transmission of motion, without the translation of matter, by means of a simple experiment. Imagine this long elastic tube to be the only medium between myself and my assistant, Mr. Porte, who holds the other end of the tube. I wish to communicate with him, and, to arouse his attention, give a sudden jerk to the tube. You observe the jerk travels along the tube, and before I can speak, it has reached Mr. Porte's hand and jerked it aside. So that something has gone from me to Mr. Porte and conveyed my idea, and yet that something is not matter, for the tube is still in my hand. It is a wave-motion which has been transmitted, and a series of jerks give rise, as you see, to a series of progressive waves.

In like manner, a bell when struck delivers its vibration to the air around, aerial pulses are thus formed which striking our ears give rise to the sensation of sound. When a body is made incandescent, the molecular vibrations do not throw the air into motion. A finer and more elastic medium is necessary. There is abundant reason to believe that such a medium called the luminiferous ether exists, the waves transmitted through which give rise to sensations of heat and light. The rate of propagation of these sound-bearing and light-bearing waves varies very much, just as we might expect from the difference in the genesis of sound and light, the former having a molar, the latter a molecular origin. In the table hung up here, you will see the rate of passage of sound and light compared. Light passing through a vacuum travels at the rate of 186,000 miles in a second, whereas sound travelling through iron only moves at the rate of 3 miles in a second, and travelling through air only moves at the rate of $\frac{2}{10}$th or $\frac{1}{5}$th of a mile in one second. We have therefore an enormously greater velocity in the case of light than in the case of sound. In fact, the velocity of sound through air is even exceeded by the velocity of a cannon ball, so that the ball will reach its target before the sound of the explosion of the cannon

is heard. You may take it in round numbers that sound travels 1100 feet in a second, whilst light travels 186,000 miles in a second. If a cannon were fired at this moment in Birmingham, which is 113 miles away, the sound of the report, supposing it were audible at this distance, would not be heard here till after the lapse of 8 minutes; whilst the light of the flash would by that time have reached the sun. Approximately we may say that sound travels through 100 miles of air, in the time that light travels through 91 million miles of ether.

This will give you some conception of the enormous difference in the rate of propagation of these two modes of motion. So too we find the relative *size*, as well as speed, of sound-bearing and light-bearing waves is vastly disproportionate. The average length of a sound wave, say the middle C of a pianoforte, is about fifty inches, whereas the average length of a wave of light, say a wave of green light, is only the fifty-thousandth part of an inch. Notwithstanding this vast difference many laws are common to both. That is to say, from the behaviour of one agent under certain conditions, we may in general predict the behaviour of the other under like conditions. And this is just what we should expect if, as there is every reason to believe, light and sound are both the products of wave-motion,—the rate of progress of the waves of each being dependent on the density and elasticity of the medium they traverse. For the element of size does not enter into the region of law. Before, however, I can render evident to you the similarity of these laws, it is necessary to have some means of revealing these sound-bearing waves, and the way in which they are affected.

This can be done by means of a delicate phonoscope—the so-called *sensitive flame*. I have here a tall burner through which ordinary coal gas can be passed from a holder, placed there for the purpose of increasing the pressure upon the gas. If now I light this tall tapering jet of gas, and place weights upon the holder, so that the gas shall be urged with greater and greater velocity through the burner, I shall presently arrive at a point at which the gas will be extremely sensitive to sound. There it is: and you observe the fact that not only does the flame

shrink under the influence of the sound, but that the slightest sound breaks down the flame, even if I go to a great distance from it. The clink of two coins makes it fall from a height of 22 inches to less than eight. The motion of the flame is not due to the impact of puffs of air upon the surface of the flame, but is due entirely to sonorous vibrations; in a previous lecture delivered to the science teachers, I have entered into the cause of this curious phenomenon. You will observe that as I speak certain sibilants are picked out by the flame, which responds to them, whereas it is not affected by vowel sounds, but *s*, *x*, *z*, a "whisper" and "hush" are all very powerful in their action. Now this flame is, in fact, an example of what may be called sympathetic vibration. If I strike this particular tuning fork and hold it over this particular jar of air, you will observe there is a loud reinforcement of the sound of the fork; that is due to the fact that the column of air within the jar is exactly tuned to the vibrations of the fork, hence there are sympathetic vibrations set up in the air of the jar which augment the sound of the fork. Now in the flame we have a sympathetic vibration of a column of gas analogous to the sympathetic vibration of the column of air in the jar, only in the case of the flame the sympathetic vibration is accompanied by a manifest change in the aspect of the flame, whereas there is no change evident to the eye in the aspect of the air within the jar. You see illustrations of this constantly. For instance, if a stone be poised on the edge of a precipice, and a series of puffs of air gently rock the stone to and fro, should the period of the motion of the stone coincide with the period of the successive puffs of air, the stone will ultimately be moved so violently that it will fall over the edge of the precipice. Thus a profound change is set up by the very slight sympathetic motion produced in the stone by gentle breaths of air; for the stone in its fall may produce disastrous results, in fact may change the aspect of nature for a considerable distance. Now just in a similar way this flame, when acted upon by certain vibrations to which it responds, undergoes a profound and entire change in its structure. It is, as

it were, a stone poised at the edge of a precipice, or like an inverted cone, a body in unstable equilibrium, easily upset by a feeble force.

I wish now to show you, by means of our flame, that just as light gradually decays as it leaves its origin, so sound also decays. If we go twice as far away from a small source of light, such as a candle, we shall find that the intensity of the light is only one-fourth as great as it was at the original distance. In the same manner sound gradually decays with distance. We find that on doubling our distance the intensity of the sound from a small source is only one-fourth as great, and on trebling the distance only one-ninth as great as it first was. You will be able to perceive the decay of sound by gradually moving a watch away from the flame. You observe now that this flame, though not as sensitive as it can be made, for in a room of this kind, subject to a great many draughts, we cannot obtain it as sensitive as in a laboratory properly prepared for the purpose, nevertheless even here we see the flame beats time to the watch, but on taking the watch further away the flame ceases to respond to it. I will now introduce a tube between the two, and we find by the renewed response of the flame that the tube has prevented this decay of sound. This in fact is the behaviour of speaking tubes and a similar law holds good for light; we may prevent the decay of light by forcing it to pass in a parallel beam through the air or by sending it through a glass rod, which is the analogue of a speaking tube, incessant internal reflection preventing the lateral divergence of the beam of light.

We must now pass on to consider the progress of sound and light waves through space. Obstructions are met with, and the waves may be either quenched or bent aside or thrown back. Now these separate motions go by the name of absorption, refraction, and reflection. Let us take the last named first. When a ray of light strikes a polished surface it is reflected in such a manner as to make the angle of reflection equal to the angle of incidence, and in the same plane with it. This is shown on this diagram (fig. 1).

If R R be a reflecting surface, and C O perpendicular to it, then a ray of light striking the reflecting surface in the direction of B O will make the angle B O C with a

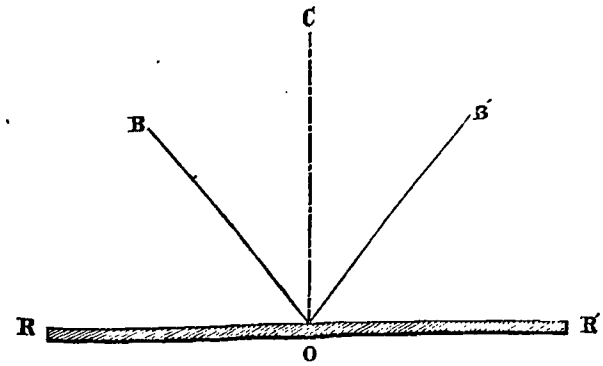


Fig. 1.

perpendicular falling on the polished surface equal to the angle C O B′; that is, the angle of reflection will be equal to the angle of incidence; and if it falls at any other angle, it will make the angle of reflection precisely similar to the incident angle. If it falls perpendicularly on the mirror, it will be reflected back in the same direction. An experiment is arranged here by which you will be able to see the equality of these angles of incidence and reflection. Here is an oxyhydrogen lamp, and in the front of it a mirror, and on the mirror is now falling a beam of light. You will see as the direction of the beam falling upon the mirror is altered, so also is the angle at which it is reflected; and the scale attached shows that the angles made by the incident and reflected ray with the normal always correspond. Exactly the same law holds good with regard to sound. I have here two tubes A and B (fig. 2) which are for the purpose of preventing the decay of sound in the manner I explained just now. The tubes are now placed at right angles to one another. In front of the tube A is a source of sound, S, muffled except so far as the open end of the tube is concerned; * in front of the other tube, B, is a sensitive

* The box is broken away in the engraving to show the interior.

flame, F, and at present you perceive no action upon the flame, though the sound (an electric bell) is going on. As soon however, as a reflecting surface, R, is placed at the base of the tubes, then we find that the sound which travels down the first tube, A, is reflected from the mirror passes back through the second tube, B, and now powerfully agitates the flame. If instead of using a polished surface, such as this mirror, I simply use my hand, I dare say we shall obtain sufficient reflection for the flame to make evident. Further you observe that a "bats-

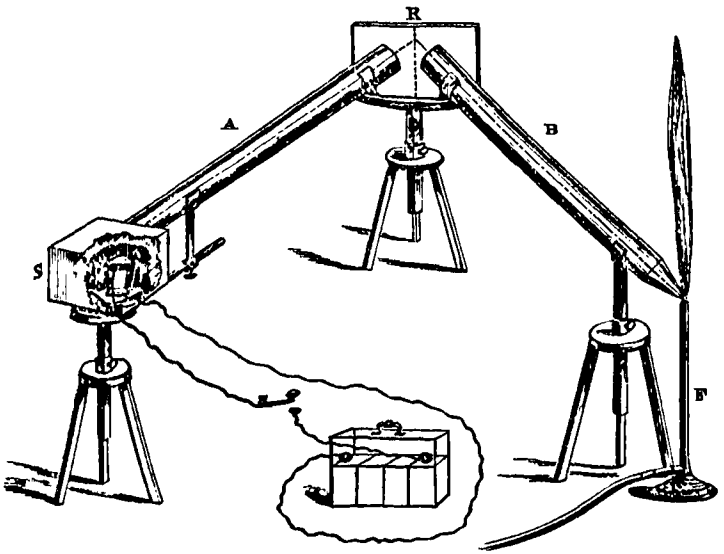


Fig. 2.

wing" gas flame, or even a sheet of unignited gas, forms a sound reflecting surface, and able therefore to replace the solid reflector R. But whatever the reflector may be, it is only in one position that it produces the maximum effect on the flame, and that position when found is such, that the axis of each tube makes corresponding angles with the surface of the reflector. Precisely the position, in fact, in which the light from a candle at the end of one tube would be reflected along the other.

So far we have dealt with plane mirrors. With curved reflectors sound is affected in the same way as light. In this diagram (fig. 3) S S′ represents two concave mirrors; at a certain spot, C or C′, in front of either mirror, termed the focus, a

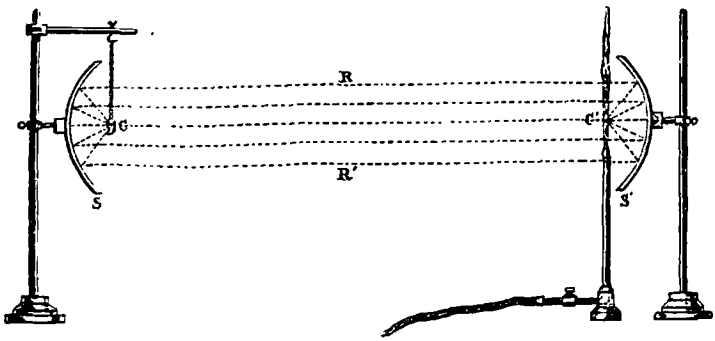


Fig. 3.

source of light or sound is placed. Divergent rays emitted from the focus and falling on the mirror, are reflected as a sheaf of parallel rays, which striking the second mirror, are there converged to a corresponding focus in front of it. As concerns light this fact is probably familiar to you all. It is easily illustrated by this large pair of concave, or rather parabolic, mirrors on the table before you. The mirrors are some thirty feet apart, and yet you see a lighted candle placed in the focus of one gives us a very bright spot of light—in fact, an image of the candle—on the little transparent screen held in the focus of the other. At any other intermediate spot the light is more diffused, and therefore less brilliant. Using the electric light in place of our little candle, the light scattered from the dusty air enables the course of the reflected rays to be readily traced by every one in this large audience. In like manner, a sound focus or a sound image may be obtained in place of the luminous one. Inasmuch as only one person at a time could ascertain this fact, we must fall back upon a sensitive flame to reveal it to us all simultaneously. Moreover, the great advantage of the flame

is the small surface it exposes, and its extraordinary power of acoustic discernment, if I may use the expression. The source of sound I shall use is a loud ticking watch enclosed in a padded case which has a moveable cap on one side. In one focus of the pair of mirrors we have a sensitive flame; and we shall find, I trust, that as soon as the watch is brought within the focus of the distant mirror, we shall have the flame beating time to the ticking of the watch. It is so, as you see. Placing the little cap over the watch immediately you find the flame is silent. Taking the cap off again, the effect is renewed. Even by turning the face of the watch towards the flame, that is away from its adjacent mirror, or moving it slightly out of the focus, the effect ceases; but placing it in the focus once more, you have evidence of the reflection of sound. I think that is sufficient evidence of the fact that both light and sound can be converged by reflection to a focus. (See *Appendix* B.)

Not only do light and sound obey the same laws of reflection, but they obey similar laws when refracted. Light on passing obliquely from a dense medium to a rare one, or *vice versâ*, is bent out of its original course, it is refracted. So also is sound. Passing a beam of light through this convex lens, you see by the smoke in the air that the light is brought to a focus, just as it was by the concave mirror. If we take a thicker lens, the light is brought to a focus much nearer. Now what I want to show you is a similar experiment as regards sound. This can be accomplished not by passing sound through glass lenses, which are much too dense for the purpose, but by passing rays of sound through convex lenses of some medium slightly denser than air, such as carbonic acid gas. I have here a collodion balloon which is being filled with that gas; this, when it is inflated, will form a double convex lens. If we now take our sensitive flame, F (fig. 4), and place the collodion balloon B between the source of sound S and the flame, we shall have the refraction of sound made evident by the convergence which takes place when the waves of sound pass through the balloon. I will place the watch at such a distance that it does not affect the flame, and then introduce the balloon, so that the focus falls on the root of the flame, which

will cause the ticking to be made evident. Taking the sound lens away there is no action; putting it between again you see a

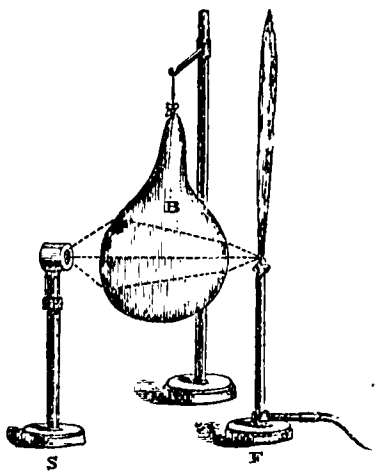


FIG. 4.

considerable action. Even with an ordinary child's india-rubber balloon sold about the streets, you may, by filling it with the air from your lungs, produce a denser medium, and obtain a very fair refraction of sound in this way. (See *Appendix* C.)

Now if we pass from the reflection and refraction of sound to the *absorption* of light and sound, we shall find that similar laws hold good here. When a ray of light passes through a continuous medium, it undergoes neither refraction nor reflection; but when this medium is broken up by any other medium of a different density, then every beam of light which enters the second medium undergoes reflection at the surface. If therefore we have a ray of light frequently passing from one medium to another, it will be frequently reflected and refracted, and every time it undergoes this reflection the transmitted ray will be weakened. This accounts for the fact that if you take some perfectly transparent crystal like rock salt, and crush it into powder, it becomes opaque in the same way as transparent ice is perfectly opaque

when in the form of snow. I have now to show you that we can produce this opacity by two perfectly transparent substances in the case of light and in the case of sound. I have here a trough containing water focussed upon the screen. In this trough I will pour a clear solution of tartaric acid, and then add also a clear solution of bicarbonate of soda. The moment they come together we shall obtain an effervescence, and the bubbles of transparent gas will be so numerous that opacity will result, and the screen, you observe, becomes quite dark. The bubbles are perfectly transparent in themselves, but owing to their number they reflect the light so frequently that it is entirely obstructed from the screen, and scattered back towards the lamp and the audience. When the bubbles clear away, the light is again able to pass through. Now I want to show you an exactly similar phenomenon in the case of sound. The apparatus consists merely of a source of sound S (fig. 5), in this case a stunted metronome, enclosed in a padded box with a hole on one side, and a row of

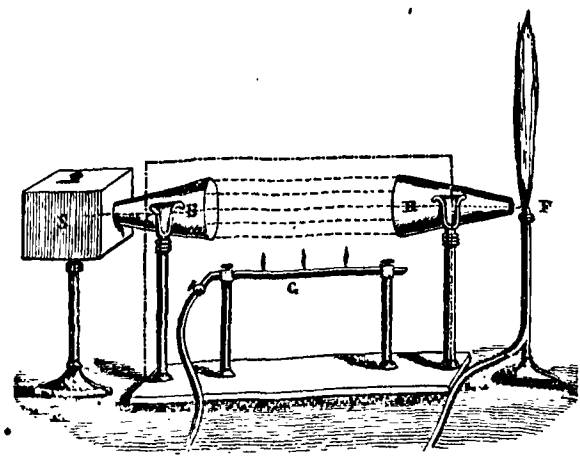


FIG. 5.

gas jets, C, between the sound source and the sensitive flame, F. Two conical reflectors of tin or cardboard, R R, are advantageous additions. The gas is now turned off, and the flame is loudly beating time to the strokes of the metronome. I will now light

three small jets of gas, and keep their flames considerably below the lower edge of the reflectors. You see the flame instantly stops its motion, though the metronome is still going on. The reason of this is as follows: the jets of gas produce columns of heated air, and these columns have a different refractive power to the cool air, and hence the pulses of sound, passing through the space between S and F, undergo reflection at every surface of hot air, and we have the sound so weakened by these successive reflections that very little or no effect is produced on the flame.* Thus we have exactly an analogous case to that which we had a moment ago in the effervescing liquid. So far then for the analogy we have traced in the *general* absorption of light and sound waves.

We might, if there was time, point out that this analogy extends even to the *details* of absorption and radiation. One reference will suffice. I presume many of you are familiar with the magnificent generalisation of Kirchhoff, who has shown us the composition of the sun from the absorption of certain luminous rays emitted by its incandescent nucleus. If a glowing gas emits a certain character of light, it will absorb precisely that character of light when cold. If, for example, a heated gas emits a certain red light it will when cold absorb a similar red light. If a body emits certain green rays when it is cold, the vapour of that substance will absorb similar green rays. Now this reciprocity, as we may call it, of radiation and absorption is beautifully illustrated in the case of sound. In fact, we obtain the clearest notion of the optical law from the parallel experiment in the phenomenon of sound. I have here a whole series of different tuning forks, two only being of the same pitch. Against one of these (A, fig. 6), which is now silent, I place a little suspended bead; now I will sound any one of these other forks which is not in tune with it, and it will have no effect upon the silent fork, but if I sound the fork, B, which

* This form of the experiment has not been published before; it is advisable to have a screen of glass on each side of the flames, as shown by the dotted line. To Dr. Tyndall is due the first arrangement for exhibiting this effect, of which the foregoing is merely a simplification.

is exactly in tune with the one now silent, the vibrations emitted by the sounding fork will be taken up by the silent fork, A, and I expect you will find the bead will be pushed away by the

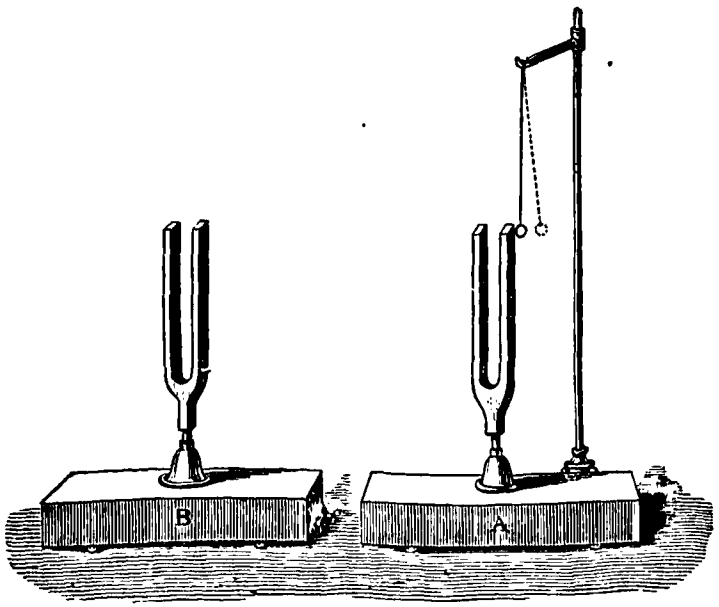


FIG. 6.

vibrations of the fork:—the movement of the bead indicating the vibrations as we saw at the beginning of the lecture. You see by the shadow on the screen the bead is agitated directly, and if I quench the sound of the fork, B, those near will hear a faint whisper of the note persisting. That is due to the vibration of A; which has taken up, and is now giving forth, the vibrating energy it has absorbed from B. I will now stop this, and fix the least piece of wax to one of the prongs of A, so as to make it a little flat. The period of the two forks now slightly differs, and you notice that there is no response on the part of A when I strike B; quickly removing the wax from A, matters are restored to their original condition, and the dancing of the huge shadow of the bead shows that A once more responds.

This interesting experiment is then the exact analogue of the phenomena of selective radiation and absorption referred to previously. A sodium flame, for example, emitting light of a bright yellow colour, corresponds to our sounding fork; cool sodium vapour, because it is capable of vibrating in the same period as its glowing vapour, absorbs light of that particular yellow character, and hence corresponds to our reciprocating fork A. Considerations such as these have led to the belief that sodium vapour exists in the atmosphere round the sun, because, from among exactly those rays which sodium itself would give us if it were burning on the surface of the earth, a portion of the light of the sun has been robbed. You will notice here how helpful is the analogy in the case of sound; how it enables us to form clearer and more definite mental pictures of these subtle luminous phenomena!

Not only, however, can a wave of light or sound be accepted by bodies in unison, but one wave can be made to extinguish or to strengthen another by its coalescence with it. Of this we may gain an excellent conception from Wheatstone's beautiful wave apparatus which I have here. You may imagine this wave-like row of beads to be either a wave of sound, or a wave of light, or a wave of the sea, and by moving this slide to and fro I can cause the wave to travel backwards and forwards. Then I have here the means of producing a second system of waves, and when the hollows of one set of waves coincide with the hollows of the other set, they will produce a hollow twice the depth, and when the crests of one set coincide with the crests of the other they produce a crest twice the height, but when a crest coincides with a hollow then we shall find that instead of getting a wave twice as great we shall get no wave at all, because the tendency of one wave will be to move upwards whilst the other moves downwards, and thus a level line, or calm is produced. Waves in this condition are said to be in opposite *phases*.

Now I want to show you the actual effect of this in the case of sound and light, that is to say, the destruction of one sound-bearing wave by another, and the destruction of one light-bearing wave by another. This is termed the *interference* of waves, and

we will try to exhibit it in the case of sound first. When I strike a tuning fork, the two prongs of the fork are sending off two sets of waves; and if I place the fork over a jar, so that the two prongs shall send waves into the jar in the same phase, we get a sound of greater intensity than would be produced by one prong; but I can place the fork in such a position that the wave produced from one prong is in a phase exactly opposed to that of the wave produced by the other prong, then the two sounding pulses will neutralise one another, and we shall have perfect silence resulting from the coincidence of two sounds in opposite phases. This I can obtain by merely placing the fork obliquely to the jar, and if I turn it backwards and forwards the sound comes and goes alternately. A similar behaviour is exhibited very well by taking two tuning forks very nearly in unison. If they are only slightly out of tune, the vibrations of one will neutralise for a moment the vibrations of the other, and we shall have intervals of sound and of silence regularly succeeding each other. These are the so-called "beats" in music, and the rhythmic swelling forth and then subsidence of the resultant sound from these two very slightly dissonant forks can be heard by every one present. By means of two pendulums of the same length, this fact of interference can be made evident to the eye. If the pendulums are both started at the same phase, they will strengthen one another; but if they are started at opposite phases, their motions tend to neutralise one another. If one is made a little shorter than the other, then they alternately strengthen and neutralise one another.

This phenomenon can be made evident by other means. Here is an apparatus designed by Koenig of Paris, and here is one I designed the other day. It is a tubular ring of brass, and if a whistle be introduced at the top, it being placed in a certain position, you will hear the sound of the whistle readily, but if you place it in such a position that the path of one wave has to travel through a longer branch than the other, a certain position will be arrived at in which the two waves will be in opposite phases, and however loudly you blow the whistle no sound will come out of the tube. (See *Appendix* D.)

It seems very remarkable that two sounds can produce silence, but we must remember that sound is motion and not matter. Further, there is only half a step between strength and destruction. Half a wave length behind another, we have perfect silence; but with two half-lengths' difference the waves coincide and they are strengthened.

We must now briefly notice the optical correspondence to this phenomenon; an analogy first pointed out by Dr. Thomas Young, who from the interference of sound inferred that in the case of light a similar action must take place. I will not read to you the description he gives, which I have here in his early memoir on the subject, but it will be sufficient for you to bear in mind that from this phenomenon of the interference of sound pulses—these beats which you have just now heard—Dr. Young inferred the existence of corresponding beats, as it were, in the case of light; that two waves of light in opposite phases ought to extinguish one another and produce darkness, and it was according to this theory that he gave the explanation of what are known as Newton's rings, or those beautiful colours that you observe in a soap bubble, the colours of thin films. Here I have on the screen an image of these Newton rings; a glass lens is laid on a level plate of glass, and the slight interval between them gives rise to these lovely bands of colour. Using red light instead of white, you see intervals of light and darkness; and these intervals of darkness are produced by two series of waves of light coinciding in opposite phases and thus neutralizing each other. We have here the analogue of our beats in music; in one case the extinctions are distributed in space, in the other in time; a characteristic difference which is even more strikingly manifest in the case of music and colour. Subsequently to Young's time, Fresnel succeeded in making two rays of light passing through slightly different paths to interfere, so as to entirely destroy one another. (See *Appendix* E.)

I am anxious now to point out what some may consider a fanciful aspect of this analogy, viz., the analogy between colour and music. You may say what can be the resemblance between

a painting by Rubens and a sonata by Beethoven; but I think you will agree with me that this relationship will become conceivable if we consider this matter for a few moments.

All the composite sounds of language, or the complex notes of an orchestra, are the result of a few simple tones variously combined. So all the colours of an autumn landscape, or on the glowing canvas of a Turner, are the product of a few simple colours variously blended and juxtaposed. Further, I need not remind you that every pigment by its own natural selection derives its colour from the colourless rays of the sun. Now the prism disintegrates white light, and sorts its constituents into a graduated series of wave-lengths, giving us that exquisite band of colour you see upon the screen, called the spectrum. In like manner we may analyse a musical composition and arrange its constituents into the sequence of notes called the musical scale. The succession of colours in the spectrum follows an invariable and harmonious order, namely—going up the scale—red, orange, yellow, green, blue, indigo, and violet. And just as any other arrangement of these colours is less enjoyable, so also is any attempt to ascend or descend the gamut than by following the sequence of notes C, D, E, F, G, A, B. Here, however, let us be on our guard, for no doubt much of the pleasure given by the spectrum is due to its delicate gradations of colour, to imitate which in music we ought to have even more notes than are to be found within the octave of Mr. Bosanquet's beautiful enharmonic organ, that is to be seen in the loan collection. The sustained sound of the common chord—as given in all its purity by these excellent tuning forks, made by Kœnig of Paris—produces on the ear a similar delicious impression of conscious repose to that which the spectrum produces on the eye; in the harmony of these forks we have, æsthetically, a *rainbow of sound.* But even here the analogy is more poetical than physical. For the musical analogy to the spectrum should be found in melody rather than in harmony; in the sequence, not the blending of tones.

Sir Isaac Newton, however, lent the authority of his great name to the consideration of the analogy between colour and music: he sought and found some resemblance between the relative widths

of the various colours of the spectrum and the relative extent of the intervals in the notes of the gamut. But this, with all respect to Newton, is acknowledged to be an unjust and fanciful analogy, inasmuch as the colour spaces vary with the material of the prism and are quite different in their relative width when seen in a 'diffraction' instead of a 'refraction' spectrum. Moreover, the coincidence between the *seven* colours in the spectrum and the seven notes in the gamut is also entirely accidental; for, though we still adhere to Newton's classification, there are really an infinite variety of tints in the spectrum.

The relationship between colour and music, if any exist, must be found by comparing the wave-length corresponding to each note of the gamut, with the wave-length corresponding to each colour of the spectrum. First let us find what range of wave-lengths is embraced by our organs of vision and hearing respectively. By proper means a good observer can see from the fixed line A in the solar spectrum, which has a wave-length of 760 millionths of a millimetre, to the line L, which has a wave-length of 381 millionths of a millimetre The latter is almost exactly one-half the former, so that the proportion is two to one, corresponding to an octave in music. Ordinarily the visible spectrum extends over an interval corresponding to a seventh in music. The range of hearing is far greater, embracing about eleven octaves. (See *Appendix* F.) But there are rays beyond each end of the spectrum our eyes cannot see, and which can be made evident by means that would take me too far from my subject to allude to now; and there are vibrations beyond the limits of hearing which can be made evident by means of a sensitive flame. Let us, however, take one octave in music out of the eleven which are audible, and compare it (in the manner previously stated) with the octave of colour, the limits of which are just visible. This we may fairly do, as the notes in every musical octave are proportional repetitions of one another.

The difficulty we meet at the outset is that there is no line of demarcation between the colours of the spectrum, but careful attempts have been made to fix the limits of the colours. The most recent and careful determination of these limits is one made

by an eminent German physicist, Prof. Listing;* a determination accurately expressed by taking the wave-length that forms the boundary of the colours on either side. Employing pure spectra, and using every precaution, Listing experimentally determined the transition places and the central region of each colour, the so-called "Fraunhofer lines" being used as landmarks. The observations were then repeated upon the normal spectrum obtained by diffraction, and were checked by the independent observations of others, and by repetitions at different times. In this way the remarkable fact was disclosed that the numbers of vibrations at the transition spots form an *arithmetical progression* throughout the entire series of colours: every succeeding colour towards the violet being due to $48\frac{1}{2}$ billions more vibrations per second than the preceding one. The number $48\frac{1}{2}$ billions expresses therefore the range of a single colour sensation in human vision. For reasons given, Listing adopts the following scale of colours—brown, red, orange, yellow, green, cyanogen, indigo, and lavender, and states as a law that this series can be physically expressed by an arithmetical progression of eight numbers, in which the last is the double of the first. (See *Appendix* G.)

The number of vibrations corresponding to the extreme limit of colour at the red end, he fixes, upon Helmholtz's and Angstrom's authority, at 363·9 billions per second, or a wave-length of 819·8 millionths of a millimetre. By adding to the former number half the colour interval—namely, $24\frac{1}{4}$ billions—the normal centre of the first colour is obtained; $48\frac{1}{2}$ billions added to that gives the centre of the next colour, and so on. These, and also the limits of each colour, are here tabulated along with the corresponding wave-length.

* Poggendorff's Annalen, vol. cxxxi. p. 564.

Table showing the wave-length λ, in millionths of a millimetre; and the vibration number n, in billions per second, of the limits of the colours of the spectrum.

	λ	n
Extreme lower limit of vision ...	819·8	363·9
Brown	768·6	388·2
Transition limit	723·4	412·5
Red	683·2	436·7
Transition limit	647·2	461·0
Orange...	614·9	485·2
Transition limit	585·6	509·5
Yellow...	559·0	533·8
Transition limit	534·7	558·0
Green	512·4	582·3
Transition limit	491·9	606·6
Cyanogen	473·0	630·8
Transition limit	455·5	655·1
Indigo	439·2	679·3
Transition limit	424·0	703·6
Violet	409·9	727·9
Transition limit	396·7	752·1
Lavender	384·3	776·4
Transition limit	372·6	800·6

The wave-lengths of any colour or note is inversely as the vibration number giving rise to it. So that if the vibration numbers are expressed by the series 2, 3, 4, and so on in arithmetical progression, the wave-lengths would be expressed by the reciprocals $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, etc. This latter is called an *harmonic* series, and according to the eminent authority of Prof. Listing, it expresses the order of the wave-lengths in the successive colours of a rainbow. Now the so-called chromatic scale in music is arranged in equal musical steps; and some may be inclined to jump to the conclusion that therefore the vibration number of its tones must form an arithmetical series like our spectrum and that the analogy between colours and tones is indubitable! The terms harmonic series in one case and chromatic scale in the other foster this

delusion. Let me, therefore, beg you to remember that there is no necessary connection whatever with music in the one case, or with colour in the other; a series of notes arranged with their vibration numbers in arithmetical progression would form simply intolerable musical intervals, and therefore no such series is to be found in any musical scale. But if instead of arranging the vibration number of our notes in an arithmetical series as 1, 2, 3, 4, etc., we arrange them in a geometrical series as 1, 2, 4, 8, etc., then we have the well-known chromatic scale, or scale of equal temperament, as it is sometimes termed. Here, then, is a vital difference between the analogy of colour and tone, the vibration numbers of one form an arithmetical series, of the other a geometrical series. Mathematicians seeking to establish an hypothesis would not, however, be deterred by such a discrepancy. The pliability of figures under such circumstances is proverbially wonderful.

I do not pretend to be a mathematician, but it is well known that the *logarithms* of a geometrical series give one an arithmetical series of numbers. Hence, as Professor Listing points out, if we take the logarithms of the vibration numbers expressing the chromatic scale in music we have a series of numbers corresponding to the vibration numbers of the colours in the solar spectrum. Physiologically and pyschologically there may be wide differences between music and colour, but here we seem to reach some sort of physical basis for this analogy. (*Appendices* H and I).

I will leave these facts in your hands, they must be taken for what they are worth, and conclusions ought only to be drawn from them with extreme jealousy and care. Let me now direct your attention to another aspect of this relationship which brings into view symmetry of form, as well as colour and sound. Doubtless harmony in the three great divisions of art—painting, music, and architecture, rests ultimately upon a physical basis.

It is possible to obtain a real optical expression of these musical intervals. This was first accomplished by M. Lissajous, who employed tuning forks, to one prong of which little mirrors were attached. By reflecting a beam of light from one vibrating tuning fork to another, placed at right angles to the first, curves of light are obtained, which curves vary according to the combination

of forks we select. These curves—Lissajous' figures they are usually called—are in fact the optical equivalent of the compound tone we hear from the two sounding tuning forks. I have here a very beautiful arrangement for showing these curves, the forks being sustained in vibration by means of electro-magnets, and a beam from our electric light being reflected from one fork to the other, and thence to the white screen behind me. In the first place we will select two forks in unison, that is the most perfect harmony obtainable, and you see upon the screen the simplest possible curve, namely, a circle gradually changing to an ellipse, indicative of the fact that the two forks are, by an inappreciably small amount, out of tune. No method of tuning approaches in delicacy this beautiful optical analysis. Next I will change one of the forks and insert its octave, we have now on the screen the figure of 8, a simple figure, but not quite so simple as the last. Next here is the interval of the fifth, and the figure you observe though beautiful is a little more intricate; the interval is not quite so perfect as the last.

Milton, when he wrote 'L'Allegro,' little imagined his words would receive so literal a demonstration as that given by the scientific discoveries of the present day. For does not this optical expression of concord remind us

> Of notes, with many a winding bout
> Of linked sweetness, long drawn out,
> Untwisting all the chains that tie
> The hidden soul of harmony.

If we now select a dissonant interval, such as the second, you note how extremely complicated is the figure on the screen; it is nothing but an interlacing network of lines of light, with every luminous thread in a state of ceaseless unrest. In fact, the complexity of the figure increases as the concord lessens, and the restlessness of the figure grows as the tuning becomes imperfect. This large diagram, which is hung behind me gives you at a glance the figures produced by various musical intervals and the phases those figures pass through. I have put over each figure the name of the interval represented and the ratio of the number of vibrations corresponding

to that interval. Thus in unison the ratio is 1 : 1, in the octave the ratio is 1 : 2, in the fifth it is 2 : 3, in the major third 4 : 5, and so on, and in the second the ratio is 8 : 9. (*Appendix* K.)

Is it not very beautiful to notice the points of contact in the harmony of colour, music, and form, the simplest numerical relationships giving us the greatest sense of pleasure? The great Euler suggested that the reason for this was to be found in the fact that we experience less fatigue in attempting to unravel simple combinations; but Helmholtz has shown the true physical basis of harmony resides in the fact that the "material ear does precisely what the mathematician effects by means of Fourier's theorem, and what the pianoforte accomplishes when a confused mass of tones is presented to it. It analyses those wave-forms which were not originally due to simple undulations, into a sum of simple tones, and feels the tone due to each separate simple wave separately," and this irrespectively of the fact whether the sound issued from the instrument as a compound tone, such as that yielded by a bell, or was the result of the mingling of two or more simple tones, such as those yielded by our tuning forks just now.

And now the question arises, is there an analogy between the eye and the ear in the *perception* of compound colours and compound tones. We fear we must answer no. The eye, so far at least as we know at present, is unable to decompose the compound systems of luminous waves, which give rise to various compound colours. As Helmholtz remarks, "It experiences from them a single, unanalysable, simple sensation, that of a mixed colour. The eye has no sense of harmony in the same meaning as the ear." But we can hardly give our assent to the great master's words, when he says, "It is indifferent to the eye whether this mixed colour results from the union of fundamental colours with simple or with non-simple ratios [that is, the ratios of the vibration-numbers, or of the wave-lengths, which give rise to the sensation of colour]. There is no music in the eye."

Is this so? If we furnish the eye with a prism and thus analyse some lovely compound colour, we shall, I think, in general, find that there is a simple ratio between the vibration

number of the constituents of the mixed colour. Take, for example, this beautiful purple yielded by a solution of permanganate of potash: the prism shows its constituents are red and blue, and the ratio of the vibration-number of these colours closely corresponds to that of a fifth in music. So we might take other compound tints. Whether such analogies are more than accidental I am not prepared to say, the matter is worthy of more exhaustive inquiry, and certainly it would not only be æsthetically interesting but practically useful if we could rely on such an analogy, and thus decide questions of taste as to the proper use of colours in decoration and dress by comparing them with their musical equivalents. Like music, colour, as Ruskin remarks, "is wholly relative, each hue in a work being altered by every hue added in other places."

In the quaint words of Sir Thomas Brown, in his 'Religio Medici,' we may say, "There is music wherever there is harmony, order, or proportion; and thus far we may maintain the *music of the spheres;* for these well-ordered motions, and regular paces, though they give no sound to the ear, yet to the understanding they strike a note most full of harmony." And I need hardly remind you of the well-known passage in the 'Merchant of Venice,' where Shakespere gives poetic utterance to the same ancient conception. Indeed, the whole order of nature looked at aright is but "linked sweetness long drawn out." Creation to the student of nature is a silent song, but one full of meaning to the reverent mind. Amid the mutability of all things we may hear the music of their march. And this surely is one of the charms of scientific inquiry. By exalting our conceptions of nature it widens our mental horizon, and increases our capacity for intellectual enjoyment of the healthiest and happiest kind. (*Appendix* L).

THE CHAIRMAN:—Ladies and Gentlemen,—I am sure you will all join with me in returning a most cordial vote of thanks to Professor Barrett for the extremely interesting lecture he has given us.

APPENDIX.

A.

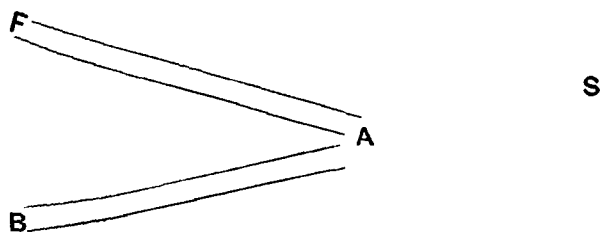
My friend, Mr. F. Millard, writes to me:—

"Philology points out many curious analogies, which show that people in all ages have instinctively associated sound and colour. Thus in Greek we have ξανθός, which denotes a yellowish, tawny colour, and some delicate thin kind of sound, which it is hard to define. In a remarkable passage (Top. 1, 15, 4, sq.), Aristotle points out that the word σουφός (spongy, porous), is applied to the intermediate sound between λευκός (white, *i. e.* clear), and μέλας (*i. e.* obscure), and hence must be "hollow," "thick." Moreover, he adds, that φαιός (dusky, whity-brown) was incorrectly applied by many Greeks to this intermediate sound. Again, in Latin *ravus* is applied to the tawny wolf and to a hoarse sound: *fuscus* is said of a dusky, swarthy hue, or of a husky, indistinct voice, opposed to *candidus*, white, *i. e.* clear. *Surdus* is deaf; but Pliny uses it of a dim, dull hue (cf. French *sourd*, applied to dull sounds): while *clarus* is clear in colour or sound. In an able article on Grimm's Grammar, contained in 'Blackwood's Magazine,' February, 1840, φαμί, φημί, 'I say,' is shown to be connected with *bhami*, 'to shine,' (Sanscrit): to give forth sound, and to give forth light, to speak and to shine being thus convertible terms. In modern languages we find our own slang expression a loud colour by the side of the stronger German 'eine schreiende Farbe,' a 'screeching' colour; or the French, 'un gilet criard.' But the whole history of this linguistic testimony to a physical analogy would demand far more than the space of a note for its discussion." In the early historic periods of the human race the absence of any sign of the appreciation of colour is well known. The fact adduced in the profound article on the Colour Sense, by the Rt. Hon. W. E. Gladstone, in the 'Nineteenth Century,' October, 1877; and in the foot note of Mr. A. R. Wallace's valuable contribution to 'Macmillan's Magazine,' of the same date,—together with the foregoing philological considerations may, perhaps, indicate that there once existed some degree of confusion or difficulty of discrimination between the impressions made on the eye and ear respectively.

B.

This experiment, together with other practical applications of a sensitive flame in illustrating the reflection, refraction, and interference of sound, were first demon-

strated by the lecturer in a paper he read before the Royal Dublin Society in January 3, 1868, an abstract of which was printed in the proceedings of that Society and reproduced, it may be convenient to state, in the 'Chemical News' and other scientific journals. In an article on the analogy of light and sound published in the Quarterly 'Journal of Science' for January, 1870, these experiments are referred to, and from that article is taken the fig. 3, in the lecture. The beautiful experiment with the tubes, fig. 2 (communicated to the Royal Society by Dr. Tyndall, on February 2, 1874,) is due to Mr. Cottrell, from whose note in the Proceedings of the Royal Society I have pleasure in making the following extracts:—"A vibrating bell contained in a padded box was directed so as to send a sound-wave through a tin tube, BA (38 inches long, $1\frac{3}{4}$ inch diameter,) in the direction BS, its action being rendered manifest by its causing a sensitive



flame placed at S to become violently agitated (see figure). The invisible heated layer immediately above the luminous portion of an ignited coal-gas flame, issuing from an ordinary bat's-wing burner, was allowed to stream upwards across the end of the tin tube BA at A. A portion of the sound-wave issuing from the tube was reflected at the limiting surfaces of the heated layer, and a part being transmitted through it, was now only competent to agitate slightly the sensitive flame at S. The sound appeared elsewhere; reflected by the heated layer, it travelled through a second tube AF; a sensitive flame at F, becoming at once violently agitated. This agitation of the second flame F continued as long as the heated layer intervened; but as soon as it was withdrawn, there was no reflection of the sound, the flame at F remaining steady, while the one at S again became agitated. Exactly the same action took place when the luminous portion of a gas-flame is made the reflecting layer, but in the experiments above described, the invisible layer above the flame only was used. By proper adjustment of the pressure of the gas, the flame at S could be rendered so moderately sensitive to the direct sound-wave, that the portion transmitted through the reflecting layer was incompetent to affect the flame. Then by the introduction and withdrawal of the

bats'-wing flame, the two sensitive flames F and S could be rendered alternately quiescent and strongly agitated. An illustration is here afforded of the perfect analogy between light and sound; for if a beam of light be projected from B to S, and a plate of glass be introduced at A, in the exact position of the reflecting layer of gas, the beam will be divided, and one portion will be reflected in the direction A F and the other portion transmitted through the glass in the direction S, exactly as the sound-wave is divided into a reflected and transmitted portion by the layer of heated gas or flame."

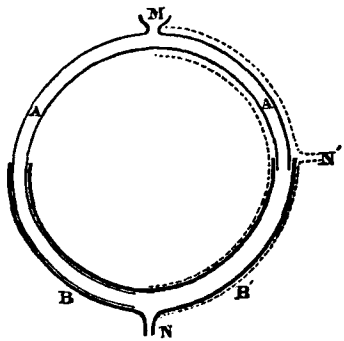
C.

When we examine more closely this analogy in the case of refraction, we see that the following difference exists:—A sound-bearing wave encountering a second medium delivers up its vibration to the particles of the new medium, whereas a luminiferous wave is, in every case, propagated by the same medium, the ether, which is assumed to pervade transparent bodies, and refraction is accounted for by the relative density of the ether within such bodies. In both sound and light, however, refraction is caused by the *change in the velocity of the wave-front.* Hence, both sonorous and luminous waves, passing from one medium to another, are bent towards the perpendicular or the reverse, according as the velocity of propagation in the former is greater or less than that in the latter. Accordingly, in both sound and light, a certain angle exists, called the "*limiting angle* of refraction," whose *sine* is found by dividing the less by the greater velocity. If the angle of incidence exceeds the limiting angle, the ray, either of sound or light, is "internally or *totally reflected,*" and is, therefore, unable to pass out of the medium. The velocities of sound in air and water are as 1090 to 4700, so that the limiting angle for these media is about $15\frac{1}{2}°$; no sound, therefore, made in the water, however loud it may be, could be heard in the air if the obliquity of the incident rays exceeded this extremely small angle, measured from the normal to the surface of the water. In the case of light, the limiting or "critical" angle for the same two media is $48\frac{1}{2}°$: the ratio of the velocities of light in air and water being approximately as 4 to 3. Many beautiful illustrations of the total reflection of light might be given; the metallic lustre, which bubbles of air in water often present, is due to this cause, or a glass rod made red hot at one end will apparently glow at the distant end, the light traversing the rod by repeated internal reflection. The internal or total reflection of sound has also been established by experiments on the Lake of Geneva.

D.

The apparatus mentioned in the lecture for showing the interference of sound bearing-waves consist of a circular arrangement of tubes, one sliding within the other. One tube A to which the mouthpiece M is fixed, is three-fourths of a circle; the

other tube, B, to which the nozzle N is attached, is half a circle, and of such a diameter that it slides freely over the tube A. When the nozzle is diametrically



opposite the mouthpiece, the path of the sound-waves is of equal length, and hence the sound from any convenient source placed near to or within the mouthpiece is distinctly heard. By turning the nozzle towards N', in the direction shown by the dotted lines, one limb of the tube is lengthened whilst the other is correspondingly shortened; the path of the waves being now unequal, a point is soon reached where the sound is nearly obliterated.

Employing a suitable source of sound, and a sensitive flame or a resonant jar as a phonoscope, an audience can perceive at once the gradual destruction of the sonorous pulses; and, moreover, the relative lengths of the two branches of the tube clearly indicate the principle of interference thus illustrated.

One instrument I made was of brass tubing, 1 foot in diameter, the one limb being $\frac{1}{2}$-inch, the other $\frac{3}{8}$-inch tube. About 18 inches in diameter would probably be the best and most convenient size. With an ordinary pitch-pipe inserted at M the experiment is very striking. A continuous blast of air should be driven through the pipe from an acoustic bellows; and the loud note heard at first is *utterly extinguished* by altering the relative lengths of the tubes. By pushing the tube still further round the note again comes out; thus the sound of the pitch-pipe can be turned on and off at pleasure. In this case it is probably the interference of two resonant columns of air, rather than the coalescence of two progressive waves in opposite phases.

E.

The principle of interference first advanced by Dr. Young, from the sound analogy, did more than anything else to establish the undulatory theory of light: a principle which Sir John Herschel remarks, "has proved the key of all the more puzzling and abstruse properties of light, and the establishment of which would alone have sufficed to place Young in the highest rank of scientific immortality."

The principle explains not only the colours of thin plates, such as Newton's rings and the colours of a soap-bubble or iridescent scum, but also the more refined and complicated phenomena known as *diffraction*, where coloured fringes are seen to accompany shadows or brilliant spectra to be produced by means of fine lines or scratches ruled closely together. It is curious, however, to note that Young was led to a false analogy in the case of the colours of thin plates; he

compares them to the sounds of a series of organ pipes. Remarking that in Newton's rings the same colour recurs whenever the thickness answers to the terms of an arithmetical progression, he notes "this is precisely similar to the production of the same sound from organ pipes which are different multiples of the same length. Supposing white light to be a continued impulse or stream of luminous ether, it may be conceived to act on the plates as a blast of air does on the organ pipes, and to produce vibrations regulated in frequency by the length of the lines terminated by the two refracting surfaces."

From the phenomena of light we pass to those of *radiant heat*, the identity of which with light is now conclusively established. Since the period of Young the phenomena of interference, double refraction, polarization, etc., of heat rays have been proved, and also the fact that transcalent bodies exercise a special selective absorption of heat rays just as coloured bodies do of luminous rays; so that every substance transmitting radiant heat has its own 'heat-tint' as it were. In the Proceedings of the Royal Institution, for April, 1852, there is the report of an admirable lecture by Prof. Baden Powell on the 'Analogies of Light and Heat.'

In connection with this subject the following striking passage from Dr. Thos. Young's Lectures on Natural Philosophy (Lecture 52), will be read with interest, when the date, 1806, on which it was written is borne in mind:—

"If heat, when attached to any substance, be supposed to consist in minute vibrations, and when propagated from one body to another, to depend on the undulations of a medium highly elastic, its effects must strongly resemble those of sound, since every sounding body is in a state of vibration, and the air, or any other medium, which transmits sound, conveys its undulation to distant parts by means of its elasticity. And we shall find that the principal phenomena of heat may actually be illustrated by a comparison with those of sound. The excitation of heat and sound are not only similar, but often identical; as in the operations of friction and percussion: they are both communicated sometimes by contact and sometimes by radiation; for besides the common radiation of sound through the air, its effects are communicated by contact, when the end of a tuning fork is placed on a table, or on the sounding board of an instrument, which receives from the fork an impression that is afterwards propagated as a distinct sound. And the effect of radiant heat, in raising the temperature of a body on which it falls, resembles the sympathetic agitation of a string, when the sound of another string which is in unison with it is transmitted to it through the air.

"The water, which is dashed about by the vibrating extremities of a tuning fork dipped into it, may represent the manner in which the particles at the surface of a liquid are thrown out of the reach of the force of cohesion, and converted into vapour; and the extrication of heat, in consequence of condensation, may be compared with the increase of sound produced by lightly touching a long cord which is slowly vibrating, or revolving in such a manner as to emit little or no audible sound; while the diminution of heat, by expansion, and the increase of the capacity of a

substance for heat, may be attributed to the greater space afforded to each particle, allowing it to be equally agitated with a less perceptible effect on the neighbouring particles. In some cases, indeed, heat and sound not only resemble each other in their operations, but produce precisely the same effects; thus, an artificial magnet, the force of which is quickly destroyed by heat, is affected more slowly in a similar manner, when made to ring for a considerable time; and an electrical jar may be discharged, either by heating it, or by causing it to sound by the friction of the finger."

To this we may add the fact, that the conductivity of bodies for sound and heat runs in closely parallel lines. In the case of wood, sound is conducted with different facility in three directions, according to the grain of the wood. This is also true of the relative conductivity of heat. And here we find another relationship appearing. The conducting power of bodies for *electricity* is well-known to be very similar to their order of heat-conducting power. In fact the analogy between the conductivity of sound, heat, and electricity, is nowhere more strikingly seen than in their rate of propagation relatively to the different axes in wood and in certain crystals.

F.

May not the wider range in the organ of hearing have arisen by a process of evolution? Among the early races of man greater advantage would be afforded by the quick detection of various sounds,—both for the purpose of procuring food and preservation from attack,—than would accrue from an increase in the range of colour sensation. If the livelihood of all mankind were in future dependent on painting or skill in spectrum analysis, we might expect to find a slow but steady refinement and extension of our range of vision; the growth of civilization is doubtless effecting the former.

G.

In the Phil. Mag. for 1848, p. 281, Brücke pointed out that *brown* should be added to the colours of the spectrum as complementary to the tint lavender-grey noticed by Herschel at the extreme violet end of the spectrum.

Brücke arrived at this conclusion from the fact, that when thin films of selenite are viewed by polarized light, brown is noticed to succeed colourless light, and on crossing the prisms so as to yield the complementary colour the brown is replaced by lavender-grey, the intensity of which is proportional to the depth of the tint of brown previously seen.

H.

Curiously enough this numerical relationship, or as some would prefer to call it coincidence, between music and colour has been examined from another point of

view. In a paper published so long since as 1845, a translation of which appeared in 'Taylor's Scientific Memoirs,' an Italian physicist, Mosotti, found an extraordinary correspondence between the ratios of the wave-length of the so-called Fraunhofer's lines in the spectrum and the wave-lengths of the notes of the diatonic scale or gamut. Mosotti used a 'diffraction' spectrum, wherein the colour spaces are arranged according to their wave-lengths, and to the Fraunhofer landmarks he added the longest and shortest wave-lengths at the two extremes of the spectrum, and also the wave-length of the brightest or central part of the spectrum. These three points, added to the fixed Fraunhofer lines B, C, D, E, F, G, H, formed a a series of ten wave-lengths, which he thus compared with the notes of the diatonic scale :—

	C	D flat	D	E	F	F sharp	G	A	B	C^2
Music.	1	$\frac{27}{25}$	$\frac{9}{8}$	$\frac{5}{4}$	$\frac{4}{3}$	$\frac{25}{18}$	$\frac{3}{2}$	$\frac{5}{3}$	$\frac{15}{8}$	2
	$\frac{1}{738}$	$\frac{1}{683{\cdot}3}$	$\frac{1}{656}$	$\frac{1}{590}$	$\frac{1}{553{\cdot}5}$	$\frac{1}{531}$	$\frac{1}{492}$	$\frac{1}{443}$	$\frac{1}{393{\cdot}5}$	$\frac{1}{369}$

	Ex.	B	C	D	Mid.	E	F	G	H	Ex.
Colour.	738	688	656	589	553·5	526	484	429	393	369

The first horizontal row of figures represents the proportional number of vibrations necessary to produce the notes of the diatonic scale, the names of which are given overhead. The numbers in the second row have the same ratio as the numbers in the first, and the denominators of these fractions represent the ratio of the wave-lengths of the respective notes. The bottom row gives the lengths in millionths of a millimetre of the luminous waves corresponding to the Fraunhofer lines named above them, and also to the two extremes or darkest parts of the spectrum marked Ex. Ex. and the central or highest portion marked Mid.

It is curious to note the fair agreement between these figures. But after all this can only be regarded as a *coincidence* and nothing more. In fact such a coincidence, where no physical relationships exist, furnishes a warning to speculative minds. For since Mossotti's time the wave-lengths of the Fraunhofer lines have been determined with far greater precision by Angström, and the following numbers show the corrections necessary: the coincidence now nearly disappears, but another starts up, if we take the line A as 100, and compare the ratios of the other lines with the ratio of the wave-lengths of the notes in the ordinary diatonic scale or gamut.

Wave-lengths of the Principal Fraunhofer lines according to Angström.

Lines.	Wave-length in Millionths of a Millimetre.
A	760
B	687
C	656
D	589
E	527
F	486
G	431
H	397

I.

In 'Nature,' for January 13, 1870, I drew attention to the analogy in the wave-lengths in the spectrum and the gamut, and though Listing's figures were the basis of my observations, I had not at the time read his paper and was therefore unaware of his conclusions which are summarised in the latter part of this lecture. The appearance of my note in 'Nature,' led to several interesting letters in that journal on the general question of the analogy of colour and music, and the arguments on both sides of this oft-disputed subject will be found ably discussed in that valuable periodical for the first half of the year 1870. Mr. James Stuart's letter (which brought Mosotti's observations under my notice) in 'Nature' for February, 1870, and Mr Sedley Taylor's letter in the following number are especially worthy of careful perusal. Mr. Sedley Taylor points out that whereas to get a good concord exact tuning is requisite—the least deviation from the right pitch being sufficient to turn a concord into a discord—to obtain harmonious colour-intervals considerable latitude is possible, any part of one colour-division producing an equally pleasant or disagreeable effect on the eye, when compared with any part of another colour-division. That is to say, there is no range of wave-length possible in musical concords, whereas there is considerable range of wave-length possible in colour-concords. Mr. Taylor thereupon shows that if instead of taking the *central* part of each band of colour (*i.e.*, its mean wave-length), as is done by Professor Listing in the table quoted on p. 454, we take into consideration the limits of each colour, that is to say, the whole range of the colour-space in the spectrum, and compare the result with the corresponding positions on the gamut, it will be found that each colour on the whole corresponds to very discordant intervals in the scale of pitch; and that only at one point (*i.e.*, one wave-length) in each colour band does it answer to a concord in music. Mr. Sedley Taylor, therefore, rejects the idea of any real analogy, and believes the correspondences to be simply numerical coincidences.

To such an authority as Mr. Sedley Taylor, I listen with pleasure and respect, but though I have no wish, nor ability, to be a special pleader on behalf of this subject, it seems to me the analogy cannot be got rid of quite so readily as Mr. Taylor suggests. As we depart from any one point in each colour-space of the spectrum, a sensible difference in tint ensues, and though the whole spectrum forms a lovely gradation of colour, yet if we remove and juxtapose the two extreme tints of any one colour-space, the effect on the eye is extremely unpleasant. Take, for example the first instance in the table given by Mr. Sedley Taylor, as opposed to the analogy, one limit of the red would correspond to the note B, the other limit to the note C, (taking the central red as C). But dark red and orange red are as unpleasant colour-intervals as the notes B and C are dissonant musical intervals, and so on with every colour-space. In fact, just as Helmholtz has shown—in the diagram whereby Mr. Taylor demolishes the analogy—that the sounds which produce the most discordant effects with the key-note lie in the immediate neighbourhood of the unison, octave and fifth, so too the tints which are most disagreeable to juxtapose with the key-note, say the colour red, lie in the immediate neighbourhood of the unison, red; the octave, lavender; and the fifth, blue. Moreover, we must bear in mind that the ear is a far more delicate instrument for the detection of intervals depending on periodic time than the eye, and we should therefore expect a more subtle appreciation of such intervals by one organ than the other. The great difference that exists in this analogy is, as remarked in the lecture, that in the case of musical concords we are dealing with a resultant sound, but which the ear, as Helmholtz has shown, analyses into its constituents; and in the case of colour-concords we are dealing with blended or juxtaposed lights. But if the eye were as highly trained as the ear, it is possible that an analysis of a compound colour would be effected by the appendages of the retina as readily as the analysis of a compound tone is affected by the appendages of the auditory nerve. Broadly viewed, there appears to be enough general agreement among the facts to justify indulgence in speculation on this analogy, and at the same time not sufficient exactitude to allow of any dogmatizing on the subject.

In addition to what has been brought forward, there are some valuable remarks on the analogy in one of Dr. Thomas Young's memoirs, 'Philosophical Transactions,' 1802; in Chevreul's work on the 'Principles of Harmony of Colour'; in a brochure by Dr. Macdonald on 'Sound and Colour,'; and, lastly and chiefly in § 19 of Helmholtz's 'Physiological Optics.' In this last, a list is given of authorities who have written on the subject since the time of Newton.

K.

There are other modes of showing Lissajous' figures than employing tuning

forks. An ingenious arrangement, made by Sir Charles Wheatstone, was shown on the lecture table; also a simple device made by the lecturer, consisting of two flat pieces of steel fastened at right angles to each other, fixed in a vice at the lower end and with a bead fastened to the upper end, after the manner of the kaleidophone. But the most beautiful and effective arrangement is that devised by Mr. Pichler, who uses two harmonium reeds with mirrors attached. Mr. Pichler's instrument was exhibited in the Loan Collection, and he most kindly took considerable trouble to show it in the foregoing lecture.

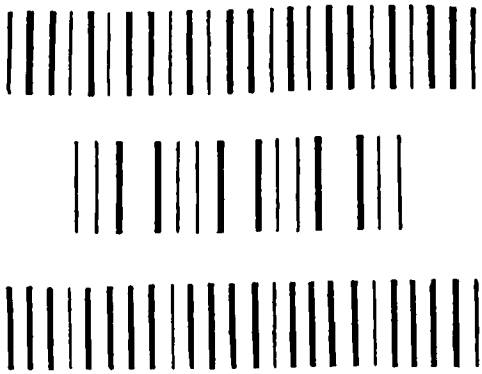
L.

The tendency of motion is to assume a rhythmic character, whether the motion be one of masses or of molecules. The shock which any body receives after the initial disturbance has subsided gives a final resultant motion which is both vibratory and isochronous. The values of the periodic times of the different types of vibration are given, as Professor Clerk Maxwell remarks in his article on 'Atoms,' in the 9th edition of the 'Encyclopædia Britannica,' by the roots of a certain equation, the form of which depends on the nature of the connections of the system. "In certain exceptionally simple cases, as, for instance, in that of a uniform string stretched between two fixed points, the roots of the equation are connected by simple arithmetical relations, and if the internal structure of a molecule had an analogous kind of simplicity, we might expect to find in the spectrum of the molecule a series of bright lines, whose wave-lengths are in simple arithmetical ratios. But if we suppose the molecule to be constituted according to some different type, as, for instance, if it is an elastic sphere, or if it consists of a finite number of atoms kept in their places by attractive and repulsive forces, the roots of the equation will not be in connection with each other by any simple relations, but each may be made to vary independently of the others by a suitable change of the connections of the system. Hence we have no right to expect any definite numerical relations among the wave-lengths of the bright lines of a gas."

Notwithstanding the conclusion thus arrived at, Mr. G. Johnstone Stoney, in conjunction with Professor Emerson Reynolds, published in the Phil. Mag. so long since as April and July, 1871, two profound papers, less known than their importance deserves, wherein "definite numerical relations among the wave-lengths of the bright lines of at least two gases" were found by experiment and mathematically discussed. The two gases were Chlorochromic-Anhydride, and Hydrogen. In a recent letter to the present writer, Mr. Stoney gives the following interesting abstract of the papers referred to.

"In the Absorption Spectrum of Chlorochromic-Anhydride, we observed 105 lines, of which we measured 31, all harmonics of a fundamental motion in the gas whose periodic time is $\frac{T}{2{\cdot}70}$ with a probable error of about $\frac{1}{800}$ of this value. In

this, T is the time light takes to advance one millimetre in vacuo. It is therefore $\frac{1}{298,000,000,000}$ of a second. Therefore the periodic motion in the gas is repeated in each molecule rather more than 800 thousands of millions of times in each second. The observed harmonics were from the 628th to the 733rd of this fundamental motion.



Absorption-spectra of Chlorochromic-Anhydride.

The intensities of the successive harmonics were indicated by the strength of the lines in the spectrum which gave in different parts the following patterns (see figure). Now these patterns would all present themselves in different parts of the series of harmonics, if the fundamental motion obeyed the same law as that of a point of a violin string struck nearly but not quite $\frac{2}{3}$ths of the length from one end. The resemblance goes extraordinarily far: however, it is not a case of identity for the *order* in which these patterns succeed one another is different in the Chlorochromic-Anhydride from what it is in the violin string.

"The law of motion is therefore in some way related to that of the aforesaid point on a violin string, but not identical with it.

"In Hydrogen there are four lines within the visible Spectrum, C, F, a line near G which may be called G', and h. Of these C F and h are the 20th, 27th, and 32nd of the series (*i.e.* the 19th, 26th, and 31st harmonics) of a motion with a periodic time of $\frac{T}{76.2}$, the outstanding probable error being excessively small. This fundamental motion is therefore repeated rather more than 22 millions of millions of times each second in the molecules of the gas.

"Now if two tubes, resembling two organ pipes, closed at one end were placed with their mouths close together and the columns of air set vibrating, the compound system would emit certain harmonics only, the rest being stifled by interference.

"If the tubes were respectively ·424 and ·576 of the total length, the harmonics which would be strongest among those corresponding to the limits of the visible spectrum, would be precisely the 20th, the 27th, and the 32nd of the series, the three that present themselves in the Hydrogen spectrum. The foregoing observations appear to warrant the conclusion that there are motions within the molecules of the two gases experimented on, which furnish either true harmonics, like those of strings and columns of air in musical instruments, or else motions which so closely approximate to being harmonics as to be undistinguishable from them, like the transverse vibrations of a free thin elastic bar."

THE POLARISATION OF LIGHT.

By W. Spottiswoode, Esq., F.R.S.

August 21st, 1876.

P. Cunliffe Owen, Esq., C.B., in the Chair.

The Chairman: Ladies and Gentlemen,—I have very great pleasure in introducing to you to-night Mr. William Spottiswoode, who has been kind enough to offer to deliver a lecture with experimental illustrations upon polarised light. Mr. William Spottiswoode is well-known to all of us here, and he is especially known to the officers of the Department of Science and Art for the very active part he has taken in the promotion of the Science Loan Exhibition.

Mr. Wm. Spottiswoode: Ladies and Gentlemen,—In addressing the Science students of this Department some few weeks ago, I explained to the best of my ability the nature and processes of that modification which light occasionally undergoes, and which is known by the somewhat technical term of polarisation. I also described the instrumental means whereby light is brought into that condition, the processes whereby it is examined, and the consequences which that examination unfolded. At that time I dwelt more particularly upon the experiments which might be performed with little, or at all events, with the very simplest apparatus, such in fact as would be within the reach of almost every one, and indeed might be constructed by any one of those here present. On the present occasion I propose rather to draw your attention to the more striking experiments which are capable of being performed by the larger and more powerful apparatus which fortunately is at our command this evening. On the former occasion I had recourse to theoretical considerations based mainly upon what is called the "wave theory of light" for explaining the phenomena in question. This evening, on the other

hand, I propose to rely mainly if not wholly upon experimental facts; and in so doing I shall endeavour to dispose our experiments so that they may bear in an intelligent way one upon another, for I assume that you would not be satisfied with a mere display of phenomena without any explanation of what we see. In reference to the apparatus itself I may mention, not in any way as glorifying your lecturer, but as adding interest to the words I may offer, that the apparatus with which we shall operate to-night is unique in size and in perfection of every kind. The construction of this apparatus has been a business of some years, and has depended upon my good fortune in having access not only to the scientific resources of this and neighbouring countries, but also to several shipments of a peculiar kind of crystal with which we shall mainly experiment, direct from that remote part of Europe called Iceland. It was only by having access at first hand to those sources of material that it has been possible to construct the instruments in question; at the same time the result has been due not only to this abundant source of material, but also to the intelligence and skill of a variety of constructors of scientific apparatus both in this country and elsewhere, to whom I take this opportunity of returning my very best thanks.

Not to spend our time in talking of mere generalities, we had better turn at once to our experiments, and the first point to which I draw your attention is the effect of crystals upon light—that in fact, will be the general purport and upshot of our lecture this evening. Now there are various ways in which the phenomena with which we are concerned to-night may be produced, but the method which we shall here use is that of passing light through crystals. We have here two electric lamps which I will use alternately. We will allow a beam of light to fall on the screen opposite and I will interpose in the path of that beam some plates of crystal. We will begin with two plates of a crystal called tourmaline, a very fine specimen of which I hold in my hand, and the object of this experiment will be to see whether or not the effect of these crystal plates is the same as that of ordinary glass or other transparent media. On interposing one of these plates you will see there is an object, a transparent medium

a plate of something between the lamp and the screen. If that plate be turned round like a wheel in its own plane, no particular effect is produced. You will see by the form of the image when it is turned round, but no particular effect is produced. We will now interpose another plate of the same crystal. The result is somewhat darker in colour than at first, but of that colour you will please on this occasion take no particular account. The plates are tingeing the light in the same way as a plate of brown or yellow glass would tinge it. If we turn them both together round in the way in which we saw that plate turned round just now, no effect will be produced, but we will now turn the first plate of crystal round, the second remaining fixed, and I beg you to notice what happens. As it turns round, the light begins to fade; and when it arrives at the position which it has now reached, viz. at right angles to its initial position, the light is absolutely obliterated. We will now turn it further round and you will see the light is gradually restored; and when it is turned round again away from the point of extinction, the light will be as bright as before; then if it is turned round so as to be at right angles, you will see the light is obliterated again, and so we might go on turning it round at a series of right angles. When it is in the one position, the light passes freely through it; when it is at right angles the light is extinguished, and when it is turned round through a succession of right angles, the light is alternately transmitted and extinguished. So far then you see the first effect of crystals upon light; and the condition in which the light is found to be after passing through that first crystal is called "polarised." The term is not one which conveys its own meaning very clearly; but, as you have seen, it means that the light has a peculiarity in some particular direction across it; and this directional character suggested the idea of a magnetic needle or an electric current or anything which has reference to a particular direction—commonly known by the name of polar,—that is the origin of the term. It is perhaps not the most expressive one which might have been selected, but it is too late in the world's history to change it.

I now go to another phenomenon. These two at first appear

isolated, but you will find that they bear on each other. I take now a piece of crystal of a different kind, viz., Iceland spar, that of which I was speaking just now, to a cargo of which I had the good fortune to have access on its arrival direct from Iceland. What we now find is that when we interpose a block of this Iceland spar in the path of the beam, we have another very striking effect produced quite different from that which would be produced if it had been a piece of glass. You will notice both now and hereafter when the light passes through it, that the spar is perfectly clear and transparent. The block is in the natural form of the crystal except that the two blunt angles are cut off in order to transmit the light more easily in that particular direction. We will now interpose it in the path of the beam of light, and when it is in that particular direction, viz., a direction joining those two blunt angles which have been cut off, and perpendicular to the two faces which are left, no particular effect is produced; but the moment the crystal is turned out of that particular position so that the ray of light passes not in that direction, but in some other different from the first, you will see two images produced on the screen instead of one; and the divergence or the extent to which one seems to slide over the other is increased with the angle at which the spar is turned from its original direction. We will turn it still further, and you will see that the separation continues. So that we have this second feature of crystals. I have taken two different crystals, because one kind is more effective in one particular kind and the other in the other particular kind of phenomenon, but they both produce the same phenomenon though not in the same degree. I will now take another block of Iceland spar cut in another manner. It is substantially the same as the piece of spar you have seen before, but it is cut differently in order to produce the greatest possible divergence of the rays and the greatest possible separation of the two images thrown on the screen; the phenomenon is, however, identically the same in principle as that you have just seen. You see now that block of spar which is perfectly colourless and transparent, and otherwise undistinguishable by the eye from either ordinary glass or other

crystalline media, will produce this remarkable effect on the rays of light. Not only will the two beams of light which go to form those images be separated, but every ray which goes to form each of those beams of light is separated, and therefore every ray of light which passes through that crystal is separated into two, and the divergence or separation is greatest the greater the turning of the crystal from the direction in which no separation takes place. There is in the crystal one direction, and one only, namely that shown above, in which no separation takes place, but the separation begins and increases as you turn the crystal round from that direction and is greatest when you have reached the right angle. This separation is called double refraction; that is to say, the bending which rays of light undergo in passing from one medium to the other, from air to glass, or from air to water, or *vice versâ*, is called refraction, and double refraction is that bending which rays undergo when each of the rays is divided into two.

Now the two phenomena, exhibited by the tourmaline and the Iceland spar respectively, which are really very distinct from one another, have a connection which we will bring to the surface immediately. If we now interpose in the path of these two beams of light our tourmaline crystal, you will see that the light will be tinged with brown as before, but, as I said above, that is not to enter into our consideration at present. On interposing the piece of tourmaline, you see that one of those rays is entirely obliterated while the other is transmitted. We now turn the crystal round, and you will see they change character; and as one of them was dark and the other was light, so, as the crystal is turned round, that which was dark before becomes light, and that which was light before becomes dark; that is to say, that this plate of tourmaline transmits the rays of one image and extinguishes those of the other when the crystal is in one direction, and it transmits the rays of the second and extinguishes those of the first when it is turned at right angles. We will turn it round once more. In that position the tourmaline excludes the one image, and you see now that the image which was bright has become dark, and that that which was dark has become bright. So that from this we may conclude that not only does Iceland spar

divide or doubly refract every ray which passes through it, but that each of those rays is polarised in the same way as was the ray which passed through the tourmaline itself, and also that the polarisation, whatever that may mean, is in one direction in one image, and in a direction at right angles to it in the second image; that is to say, if we examine or, as it is technically called, "analyse" the light after it has passed through the block of Iceland spar, by a piece of tourmaline, we find that whatever effect is produced upon one image in one direction is produced in the other image in a direction at right angles to the first. The light of one set of rays is said to be polarised in one direction and that of the other in a direction at right angles to it. So that if you suppose that the rays of light which fall on one image had some peculiarity in a vertical plane, the rays in the other image would have a similar peculiarity in a horizontal plane; consequently as the crystals are turned round the directions in which those peculiarities lie are similarly turned round, and that which was horizontal becomes vertical and *vice versâ*.

There is one other very important fact relating to these two rays which we shall be able to bring out by another piece of Iceland spar. I have here a sphere of that substance, probably the largest and purest in existence. If this were a sphere of glass it would simply act like a bulls-eye lens on the lantern, and produce an enlarged image of the aperture on the screen, but you will not be surprised if when you look at the image transmitted through and magnified by this sphere of crystal, we shall find two images on the screen instead of one. Now what I want to prove is that of the two sets of rays which pass through crystal and notably through the Iceland spar (but it is the same through all crystals), one moves faster than the other. Now considering the extreme velocity with which light passes through air, glass, crystal, water, or anything else, viz., at the rate of some 190,000 miles per second, you will perhaps think it is a somewhat bold assertion for me to say that I hope to show you visibly that one of them moves faster than the other; but so in fact it is; and if you reflect for a moment, I think you will agree with me that the proof is possible. If we have two bodies moving with different velocities through

the same medium, that which moves the fastest is less diverted from its course by the resistance of that medium than is the one moving slower. If, for instance, two balls be thrown into the air, one with greater and the other with less velocity, we find the resistance of the air acting more powerfully upon the slow one than upon the fast one. So likewise if we vary the experiment in any way, we find the resistance of the medium to the moving body has more effect on the slower one than on the faster one. Now if we see on the screen two images, one larger than the other, what does that mean? It means that the rays forming the larger image have been more diverted from their course, brought to a focus, and spread out again more than those forming the smaller image; because those forming the larger image are more oblique than those forming the smaller one. You see one lies within the other; so that one is smaller than the other. Thus we may fairly say that those rays which form the larger image have been more diverted from their course in passing through the same crystal than those forming the smaller image; in other words, that one set of rays has moved slower through the crystal than has the other set of rays. We will now allow the rays of light to pass through this ball of crystal and see what happens. There are two images formed; by turning them round one is spread over the contour of the other and is clearly larger. We may therefore fairly conclude that the rays from this larger image have moved slower through the crystal than those which form the smaller image.

We have now therefore established three distinct principles: one, that crystals have the effect of giving a certain directional character to a ray of light, directional, I mean, across the ray; secondly, that they divide the rays into two; and, thirdly, that each of those branch rays have that particular character; but in whatsoever direction be that character developed in one ray, then it is at right angles to it in the other. I spoke just now of one being vertical and the other horizontal, but that is quite immaterial, because one may be oblique and the other may be oblique, but whatever may be the direction of the one the other is at right angles to it; so that if I turn the crystal round, those rays will have their directional character turned round with the crystal;

the rectangular relation in the two rays is however always preserved. We will just interpose that analysing or examining plate of tourmaline and we shall see the effect on the screen. The effect is here rather complex, and I think in fact it would be better to turn the crystal round out of its present condition so as to reach 90°. You see that one of these images is extinguished by this position of the tourmaline; and now if the piece of tourmaline is turned to 90° or at right angles, the other image is extinguished; so that we here recognise our old features of the two sets of rays or beams; and it is the same whether it is formed by a block of crystal, by a plate of crystal, or by a sphere of crystal. The difference of velocity between those two sets of rays is called the retardation of one set of rays over the other.

These are the three principles which are involved in the phenomena of the polarisation of light. They are a little troublesome and a little difficult, but there they are; and you have seen at all events the evidence for them. I do not hope, or ask you to carry away in your minds an excessive quantity of what you have heard and seen to-night, but I hope when you come to study the subject, and perhaps experiment for yourselves, you will be able to remember these three illustrative experiments which I have had the pleasure of bringing before you.

We now come to another instrument, very generally used, for bringing the light into the condition of which I spoke just now, viz., polarisation, and for examining or analysing it. We saw that the block of Iceland spar produced two rays, and that each of those rays was polarised or had this peculiar directional character across its own line of passage. Now we do not always want two rays; in fact, the second ray is sometimes more troublesome than not, and therefore a certain Englishman, a good many years ago, named Nicol, devised a contrivance whereby he put together two pieces of Iceland spar, or rather took one piece and cut it in half, and having interposed a film of Canada balsam in the plane of cutting, put them together again. The general effect of which was to throw one of the beams of light, one set of rays, out of the field of view altogether, and to allow the other one alone to pass through to the eye or to the screen. The exact method of construction you will

find in the books, and I will not detain you by describing it here; but the general effect of it is to do the same thing as the block of Iceland spar did and as the double image prism did,—divide the rays; only it gets rid of one of them and allows the other to pass through. The instrument is called a Nicol's prism; and in the same way as at the outset we took two plates of tourmaline, that is to say, two similar crystal plates, one for bringing the light into this peculiar condition, and the other for examining whether or no it is in that condition—one for polarising and the other for analysing, as it is called; so here we take two Nicol's prisms, one for polarising or bringing the light into that condition, and the other for examining or analysing it. The advantage of those two prisms over the tourmalines is that we have now no colour to disturb the appearance of the phenomena; and inasmuch as colour is one of the peculiar effects produced by plates of crystal when submitted to light of this particular character, the absence of colour in the instrumental part of the apparatus is of great importance. But in order now that we may recognise that we are dealing with instruments having the same power as the first, I will pass a ray of light through the apparatus. You see now that the beam of light passes through the whole train of apparatus; it passes from the lamp through the polarising Nicol's prism, then through a lens which focuses any object you please on the screen, and afterwards through a second or analysing Nicol's prism. If what I said before be the case, viz., that the light after passing through the first prism is polarised, and also is capable of being analysed or examined by the second prism, then it ought to be possible to repeat that phenomenon we saw before by means of this apparatus; and in order to test that we will turn the polariser round, and you will see that it turns round by the motion of the image of an arrow attached to it, which I have focused on the screen. You see as it is turned round, the light gradually fades; and if we go on turning, we shall find that when it is turned to a right angle, that the light is obliterated. We will now continue our turning through the second right angle, which you will be able to notice by the motion of the arrow about its centre, and when it has reached the direction opposite to its original direction, the light is restored to

its full brilliancy. In order to complete the entire revolution we turn it again, and when it has reached the second right angle, the light is again obliterated; lastly when it is turned through the fourth right angle, and arrives at its original position, the light is as bright as it was before. In this I hope you will recognise the same phenomena as at first.

Now I must refer briefly to the term the "wave theory of light." A ray of light is supposed to consist of a series of waves or undulations of an extremely elastic ether which pervades all transparent if not also all other bodies. The vibrations to which those waves are due are always at right angles or perpendicular to the ray of light. In ordinary light they may take place in any direction so long as they are perpendicular to that ray, but in polarised light they are all brought into one direction. Thus, if we consider the ray of ordinary light, the vibrations at any point of the ray may take place in any direction whatever, provided only they are confined to a plane through the point perpendicular to the direction of the ray. But according to that theory the effect of any polarising apparatus is to bring all those vibrations into a single plane containing the ray. Suppose for a moment that plane to be vertical, then, if we turn the crystal which so operates upon the light through any angle, the plane in which those vibrations are contained turns round with it. The effect therefore of the polariser and analyser is not very difficult to see. Suppose for a moment that the polariser, our first Nicol's prism, will transmit in its original position only vertical vibrations, then if the second Nicol's prism or analyser be similarly placed, it will receive and transmit vertical vibrations; but suppose we turn the polariser round so that it will only transmit oblique vibrations, then the oblique vibrations will fall upon the analyser, but only a portion of those oblique vibrations will pass through; and inasmuch as those oblique vibrations may be considered as partially vertical and partially horizontal, then the vertical portion of them will pass through, but the horizontal portion will be extinguished. If we turn the polariser round until it is at right angles to its original position, so that the whole of the light consists of horizontal vibrations, then there will be no vertical vibrations whatsoever falling upon the analyser and no

light will be transmitted. That is the general idea given by the wave theory of the phenomena which you have seen just now, and though I beg you not to try to carry away more than you can easily retain, still I hope you will have grasped the idea of a peculiarity in a particular plane being due to the polariser, and of that plane being capable of being turned round with the instrument itself.

The great point we are now coming to is this. If there be interposed in the path of the beam of light another plate of crystal, a crystal between the polariser and the analyser, what will take place? What we have seen on a larger scale here will there take place on a smaller scale. In the first place every ray which falls upon that intervening plate of crystal will be divided into two. The plane of polarisation of the one set of rays will be in one direction, and the plane of polarisation of the other set of rays will be in another direction. No matter what that direction may be, there will be two sets of rays, one polarised in one direction, and the other in a direction at right angles to it; and not only so, but, as you saw with the sphere of Iceland spar, one of those rays will move slower than the other through that crystal, so that on emerging one set of rays will be slightly retarded or shunted behind the other. After emergence from the plate of crystal, we have two sets of polarised rays. These will fall on the analyser or second Nicol's prism, and those parts of the vibrations which are vertical (supposing the analyser so placed as to transmit vertical vibrations) will alone get through the analyser. Suppose them to be oblique or at an angle of 45°, one in one direction and the other in the other. Then one part of each will pass through the analyser in the same vertical direction, but those two sets of waves will no longer be coincident as they were when they began; the crests of the one will no longer be coincident with the crests of the other, and the hollows of the one will no longer be coincident with the hollows of the other; there will be a separation; the crests of the one will be behind the crests of the other, and the hollows of the one will be behind the hollows of the other, and the effect of that will be that the two sets of waves

will interfere with and partially or even entirely obliterate one another. Suppose for a moment there were passing before you from one side of the table to the other two sets of simple wave motions, on a surface of water, and that in virtue of one set of waves the water is raised up from its natural level to a certain height; then supposing there came another wave motion upon that water so that the crests of one wave motion coincided with the crests of the other, then the two wave motions would assist one another, and the water would be raised to double the height; supposing, however, that the second set of waves did not coincide with the first, but was so far apart that the crests of the one coincided with the hollows of the other, then the water which was originally at this level would be raised to the height of the wave in virtue of one set of waves, and it would be depressed to the same extent in virtue of the other; that is to say, the action of the two wave motions would be to oppose one another; the one raising it above the level and the other depressing it below the level, each to the same extent, and the water would remain at the original level, quite unmoved; that is to say, the effect of two sets of waves where the crest of the one coincides with the hollow of the other, and *vice versâ* would be to neutralise and obliterate one another. The subject is rather troublesome to follow but the effect on white light of it would be this. White light, you know, is composed of light of various colours. These colours are due to the difference of wave length, and consequently if the white light is divided into two sets of rays, one set of which is retarded a given distance behind the other, then the distance by which one wave is retarded behind the other will be a smaller proportion of the wave length in the case of the red than in that of the violet waves. The red waves are the long ones, and the violet waves are the short ones, and therefore the same common distance of retardation will be a larger fraction of the short wave than it is of the long wave. So that there may be one set of waves which will be retarded exactly the distance between the crest and the hollow; when that is the case, that particular colour will be obliterated, and there will remain visible to the eye the remaining assemblage

of colours, which, together with the obliterated one, would go to form white light.

We will now, after that rather technical explanation, make the experiment; and I hope presently to show you by a second experiment that what I have described is actually the case. We have here a ray of light passing through the polariser and analyser, forming on the screen a disk on which there is seen the outline of a little crystal plate. The light passing through this plate is divided into two sets of vibrations, one in one direction and the other at right angles to it. But it is now so placed that one of those sets coincides with the vibrations which the two Nicol's prisms, the polariser, and the analyser can transmit, and no effect is produced; that is to say, one set of waves is transmitted, the other is entirely obliterated, and we see nothing of it; but the moment we turn the plate round we see what happens. How is that beautiful green colour now before you produced? The vibrations transmitted by the analyser in its present position are vertical, while those transmitted by this plate are at 45°, or we may say north-east and south-west. Of these two, one set is retarded behind the other, and it so happens that the red waves are retarded just one half-a-wave length, or the distance between a crest and a hollow; these are obliterated, and we have left remaining on the screen an assemblage of colours, in which the green is predominant. If we now turn the polariser round, or the analyser, it matters not which, you will see that the vibrations transmitted by the analyser are exactly coincident with one of the sets transmitted by the plate. If we turn it still further round to a direction at right angles to the first, we shall find that instead of having green we have that beautiful red colour which is now thrown upon the screen. The real reason of the change of colour, as I hope you will see presently, is that the rays which are extinguished in the one case are complementary to those which are extinguished in the other. Now the green which was at first predominant is extinguished, leaving the general impression of red upon the screen. The exact colour which is retarded a half-wave length, and therefore extinguished, depends of course upon the thickness of the

crystal, because inasmuch as the retardation depends upon the amount of crystal through which the light passes, if we have a thicker plate, the retardation will be greater, and instead of retarding half a short wave length we shall retard half a longer wave length; in other words, instead of extinguishing a ray somewhere near the blue or violet end of the spectrum, we shall extinguish one somewhat nearer the red end. If, therefore, we have a series of similar plates of different thicknesses, each thickness will extinguish its own peculiar coloured ray, and have a different residuum upon the screen. In illustration of this we will interpose in the path of the beam a compound plate of crystal, formed of compartments of different thicknesses. It is now placed in the neutral position, and we begin, as before, with the polariser and analyser differently placed. We now turn the crystal through the angle of 45°, and we see the beautiful arrangement of colour thrown upon the screen. We have now only to turn the analyser through 45° to extinguish those colours, then through a second 45° to produce the colours complementary to the first. The principle is precisely the same as in the smaller experiment, although the results are a little more complex and I think you will agree with me in saying to that extent more beautiful. We vary the experiment by taking instead of a plate which is composed of various compartments ground down and polished with great nicety, a plate of the same crystal which is roughly split and not polished down at all. This will give us a variety of colours, the principle, however, is precisely the same; there is no novelty or complication in the matter, but merely that these different parts are composed of films of different thicknesses. We turn the analyser round, and again the colours fade, and are replaced by the complementary colours as you see them upon the screen, according as the angle of turning amounts to 45° or 90°.

There is a question which you will very likely ask, which is this, we began with a thickness of plate which extinguished the violet or shortest rays, then we found a plate a little thicker was able to extinguish blue, the green, the orange, and so on, until we have a thickness which will extinguish the red; but now suppose we take a still thicker plate, what will happen then?

The answer is, we shall begin again, and a plate thicker than the one which extinguishes the red will extinguish the violet again. But there is something more in it than that, the explanation of which I will reserve till presently. I will now interpose in the path of the beam of light a wedge-shaped piece of the same crystal, so that we pass by imperceptible degrees from the thinner end to the thicker end. If what I have stated be true, we ought to have a kind of rainbow effect, viz., all the colours in succession in a sort of band like a rainbow across the field of view, and after that the colours ought to begin again, and be repeated over and over again as far as the thickness of the crystal will allow. We will now try the experiment. At one end we see the green pass through a series of colours more or less pronounced until we get to the red, and then we see the series is repeated over again. There you see a series of rainbow effects showing that the colours pass through a series of gradations and are repeated as soon as the cycle is completed. Again, we may take two wedges which we can put close together so as to increase the angle. We can either put them so as to place the thick end to the thin so as to neutralise each other; or the two thick ends together so as to increase the effect, and then the number of cycles is doubled, or nearly so.

I should call your attention to the fact that when the number of cycles of colour is increased, the brilliancy of each is diminished, and I shall have a word or two to say on that fact later. I will now vary the experiment by taking, instead of a wedge-shaped piece of crystal, a concave piece so that the centre is thinner than the circumferential parts. By so doing we have the same thickness all round at a certain distance from the centre, and therefore the colours are ranged in circles round about the centre. As we pass from the centre towards the circumference, we pass from a thinner to a thicker plate, and we get these variations and repetitions of colours. Here you see what I noticed in the last case, perhaps more plainly that the intensity of colour is greater towards the centre and feebler towards the circumference. The reason of that I hope to explain in a few minutes.

I will now try to make an experimental examination of the

mode in which these colours are produced, so as to test the truth of the explanation furnished by the wave theory. We will use for this purpose that new optical instrument which has proved so fruitful in the hands of philosophers of modern days, namely, the spectroscope. I have now placed in front of the beam of light a slit, giving a thin slice of the light which we have been using. I will introduce in the path of the light a prism which will have the effect of dispersing the rays of light, throwing them all more or less to the right, but the shorter rays more to the right than the longer; in fact, we have on the screen a complete spectrum. We will now introduce a plate of crystal such as we have just used. I will remove the prism for an instant to let the beam of light pass on the screen; we now have a sort of reddish tinge of light passing through the crystal. I repeat what I said before that the colour complementary to the red somewhere about the green would be found to be obliterated. We will now throw the spectrum on to the screen, and there you see that the bluish end of the green is obliterated; the spectrum which was before continuous in light from the red to the blue end is now separated by this dark band in the green, so that the bluish part of the green has been effectually obliterated, and we have left on the screen the colour composed of all the other colours of the spectrum. It is therefore true that the colour transmitted is due not to the addition of any colour to white light, but to the subtraction of one of the elements which go to make white light. We will next take a still thicker plate of crystal, and you will, I think, see that the image upon the screen is but very little tinged with colour at all. You may fairly suppose from what you see there that little or no effect was produced upon the light by this crystal, nevertheless, the moment we interpose the spectroscope in the path of the beam, we shall see that a great deal has happened. We have in fact not only, as in the first instance, one band of darkness in the spectrum, but we have more than one—one near the end of the red, so that the orange and yellow is obliterated, and we pass almost abruptly from red to green; furthermore that there is a band of shade again showing that in that part of the spectrum in which green verges upon blue, the light is obliterated, and furthermore you can see

a shade in the blue. What happens, therefore, is this, that we have obliterated three parts of the spectrum, leaving four components contributing to the remaining light. In the former case we had but two sections of the spectrum, here we have four sections. Now, white light is made up of portions from every part of the spectrum, and the more nearly we approach to every part of the spectrum, or in other words, the greater number of parts from which we collect the light, the more nearly shall we transmit the white light; and if we take one more plate which is still thicker, we shall find that the image on the screen is paler than at first, and that the parts of the spectrum from which that light has been taken are more numerous than they were before. On forming the spectrum, you see there are over it many bands of obliteration, or absorption bands as they are called; and consequently many components whose combination forms the image you saw on the screen. I hope you will agree with me in thinking that this confirms the theoretical explanation I gave just now, and furthermore explains the comparative feebleness of the tints as we pass to thicker and thicker parts of the crystal.

Before leaving this part of the subject I should like to notice two other specimens of the same crystal where we have not the merely simple figures you saw just now, but compositions wherein a certain figure or design has been cut out; the different parts of the design are made up of different thicknesses of the crystal, so that we have really something like a picture shown on the screen. This is a design which was constructed by the late Mr. Darker, who was himself not only an artisan, but a true artist in these things. On turning the analyser round, you see the same image in the complementary colours.

We have not many minutes left, but there is one other class of phenomena which so closely resembles these, that if you can give me permission I should like to call your attention to them. All the phenomena you have now seen are due to the internal structure of the crystals themselves, and a crystal differs from an ordinary non-crystalline substance in having a structural character, whereby its composition is different in one direction from what it is in another. It therefore naturally occurs to our

minds whether, if we were to take some particular non-crystallised body and submit it to pressure or squeezing or distortion of any kind, we should not produce, at all events temporarily, on the internal parts of the substance something approaching to a crystalline character. The simplest possible experiment will show that this is perfectly feasible. I have here a simple bar of glass, about half an inch broad, which I will place in the path of the beam of light, between the polariser and analyser so placed as to cut off the light; the bar has no effect. Now, I will take it between my finger and thumb, as if I were going to break it, and the slightest pressure will I think reveal it to you. The moment I press it, the internal condition is in a state of strain, quite sufficient to affect the light. In particular, you saw two parts bright towards each side, whilst the centre itself was dark. My assistant has now placed this in a vice so as to heighten the effect, and I would direct your attention to the rod of glass which is now being bent. Since one side is in a state of compression, and the opposite side is in a state of tension, there must be some intermediate part towards the centre, in which there is neither one nor the other, the glass in that part remains in the same condition as if there was no force applied to it, and therefore the field remains dark. That is the meaning of the dark band in the centre. We will now screw up the vice, and not only do we get the light more brilliant towards the edges, but we get tinges of colour so that it does begin to resemble the crystalline effects we saw just now. The moment the pressure is released the light fades, and the glass acts as an non-crystalline body. I have here another piece of glass in a frame, and as I increase the pressure you see the colour produced, showing that the pieces of glass became more and more strongly crystalline than before; gradually reducing the pressure, the colours die away and fade until at last the field becomes quite dark.

What we have done by mechanical power can be done to a much greater degree by other powers which we have at our command. Many of you will have heard of molecular forces, how they act at inconceivably short distances, and with inconceivable intensity. Suppose then, instead of relying on mechanical

pressure or squeezing, we have recourse to heat, and heat a piece of glass until it is perfectly soft. That being done, suppose we place it in a mould so that the outside shall cool before the inside. The outside becomes a rigid frame, to which all the interior parts have to conform as best as they may, pushing, squeezing, and straining themselves; and as it cools the interior of the glass is in a state of strain or pressure. When it is cooled in that condition —unannealed as glass-makers call it—it is in a very brittle state, but still in a condition to produce these extraordinary effects on rays of light passing through it. We will now introduce a circular piece of glass of this kind, and you see how the lines of pressure are arranged, as you would naturally expect, in circles round about the centre, the pressure being uniform throughout the entire contour, has affected the figure symmetrically. Suppose, instead of a circular piece of glass, we take an elliptical piece, you see how complex the figure becomes and beautiful as that figure is, it is still more interesting to think what that figure tells us, namely, of the extraordinary complexity of the pressures and expansions which are perpetually going on within that glass due to the cooling of the outside, and of the necessity of the inside conforming itself to its external barrier as best as it can. We may vary the effect by taking other forms of glass. There is a square block which gives a magnificent figure, but you will see the pressures and tensions within that block are by no means so simple. As we turn it round, we get different manifestations of these forces, for they are really what we may call coloured diagrams of the forces which are going on within the block of glass. There are several other forms which I will just throw on the screen, and one of the most splendid perhaps consists of two rectangular blocks of glass, and you see what a system of forces that tells us of, and we may well wonder how complex a subject the whole is. I know of no other process whatever whereby we can get placed visibly before our eyes an image or picture of the forces which are going on there; and in fact some years ago, a Frenchman, with the ingenuity of his nation, conceived by such an arrangement as this a method of studying the internal forces going on within different blocks of

material under different mechanical forces. That is one of the purposes to which this has been applied. One might continue these experiments to almost any extent and vary them in many ways, but there remains only one matter to which I should like to draw your attention for a moment, going rather back to where we were before, namely, to crystalline structure, because in this experiment if we succeed, I hope we shall combine the two. There are some substances which at one temperature are uncrystalline, and at another are crystalline. Here I have a specimen of some of the bodies called fatty acid, which are smeared on glass. My assistant has now warmed it, and if we succeed in cooling it sufficiently rapidly, you will see the crystals forming themselves on the glass. There is nothing visible at present except that you may see the circular ring, which indicates the region within which this crystallisable substance is now lying. You now begin to see crystals forming from the outside, and the moment they are formed they so act on the light as to produce colours similar to what you saw before. Thus you see these crystals forming and producing this beautiful optical effect, grouping in from the circumference towards the centre. And you may watch not only the actual formation of the crystals before your eyes, but the very modification which the polarised light undergoes, as it passes through the crystalline as compared with the uncrystalline medium.

These experiments, as I said, might be carried to almost any extent, and if I shall have succeeded in so far interesting any of you as to induce you in some way or another to take up some part of the subject, our hour will not have been spent in vain. The subject in its integrity is undoubtedly a very complex one, and on the scale in which you have seen it this evening it can hardly be hoped very often to be produced; but nevertheless the student may himself with his own eyes see all these magnificent things although on a smaller scale. I sincerely hope that some of you will at all events try, after reading a little about this subject, to repeat these experiments yourselves. It is given to some of us to add to our knowledge of this knowledge by actual research and by scientific discoveries; to others, although it is not given to

extend the field so widely as the first, it is given to repeat, to verify, or even occasionally to disprove, to illustrate, to modify, and to vary the experiments which have been made by others, and by these modifications and illustrations to add very materially to our body of knowledge and to our intelligent apprehension of the subject. There is no doubt that the tendency in everything is to division of labour. Some, no doubt, beside their main occupation of life, are more attracted towards politics, others towards religion, others towards social questions, others towards scientific subjects; and I doubt not that as the world goes on, that division will become wider and more marked than ever. Nevertheless, while on the one hand none of us are absolutely separated from, or can in any way divest ourselves of our duties and our relations in respect of these other branches, we may still in some degree verify the old saying, "that the perfect man ought to know something of everything and everything of something;" and therefore whether our bent of mind, whether our main purpose of life, whether our associations or affections lie in one direction or another, I think we ought at all events to feel it our duty to try to extend their range and leave the world a little better than we found it.

The CHAIRMAN: Ladies and Gentlemen,—Allow me to remind you that this is the 27th lecture of what has been so appropriately termed by a gentleman who has reported these lectures in the "Times" newspaper, "Free Science at South Kensington"; and I am happy to have an opportunity of expressing in public, what has been expressed by my lords, the thanks of this department generally for the assistance which has been given so freely by gentlemen of European distinction, such as Mr. Spottiswoode, who has been kindly interesting us this evening. It was felt that unless there were lectures and explanations of the wonderful objects that have been brought together from all parts of Europe, this collection could never have any life; and it has been really these lectures which have encouraged the hope that what has been brought together, as the Science Collection of 1876, may be the foundation of a permanent collection which may do honour to this country. The South Kensington Museum is attached

to the Science and Art department; much has been done for Art and the public have at length appreciated what has been done. But it has been our duty on this occasion, and my lords called upon us to do it, to give all our energies to the promotion of Science and to the formation of a great collection of scientific apparatus which should not only rival, but surpass the *Conservatoire des Arts et Metiers*. We have been greatly assisted by those gentlemen who have so kindly come forward to give us these lectures—unfortunately there are but two more—and we are encouraged to hope that a permanent collection of the kind which is contemplated will always be heartily supported by those gentlemen whose lives are taken up in the studying of various branches of Science. I will only now express what I know you all feel, our most cordial thanks to Mr. Spottiswoode for the very interesting lecture which he has been kind enough to give us.

STANDARD WEIGHTS AND MEASURES.

By Mr. H. W. Chisholm, Warden of the Standards.

August 28th.

J. Scott Russell, Esq., F.R.S., in the Chair.

The Chairman: Ladies and Gentlemen,—In taking the chair on this occasion I believe it is expected of me that I should make a few remarks upon the peculiar position of this lecture as being the last of a series of Free Lectures on Scientific Subjects given to the public in this great hall, in which for the first time you see collected a large museum of instruments and apparatus which represent the great triumphs of human intellect and human science made during the last few centuries, and especially characteristic of the marvellous progress of science during the century in which you and I have the good fortune to live and work. I will not occupy your time with any remarks upon the subject before the lecture, but the peculiar position of this as the last of a first series of such lectures, and, as I hope, the beginning of what I call an interminable series of such lectures, is in that respect so critically important that I will not neglect my duty, but will make a few remarks on the subject at the end of the lecture. In the meantime, allow me to introduce the Lecturer to you as the man who in all England is most competent to speak to you on this subject; and allow me to introduce the subject to you by saying that possibly it may seem to some of us as not very important that a yard measure should be so wonderfully accurate as to require a great national institution and a number of great national institutions in other countries to keep the yard measure all right. But let me tell you some of the consequences of not having the yard

measure all right. The whole of the navigation of the ocean depends on the accuracy with which we have measured the earth, and if we have made any blunder, our ships are wrecked accordingly. Now let me tell you that if in English yard measure—for we have nothing else to measure the whole big world with but a yard stick—if in the yard stick with which we have measured the earth, we have made an error of a thousandth part of its length, the place of our ships at certain parts of the earth will be 24 miles wrong. But we have nothing but a yard measure to measure the Heavens and the stars with, and if the measures of the earth are so put out by our little invisible blunders, let me ask you what can be the conditions of the measures of the Heavens and of the planets? because where our earth is miles in diameter, their orbits are millions of miles in diameter. What then will be the state of our calculations if we have made any blunder? I will tell you. The French made a little blunder. They invented the metre, and they thought they had made a grand invention. They told all the world that they had not made the metre out of their own heads, but that they had taken the exact dimensions of our globe, that they had taken the exact round measure of it, that they had divided that into four quarters, and that of those four quarters they had taken an exact $\frac{1}{10,000,000}$ part, and that that $\frac{1}{10,000,000}$ part of the quarter of the girth of the globe was the standard French metre to which all nations must for all time coming accede, and we would have done so. But what do you think we found? That the metre is wrong, and the other day we found out that the French measurement of the earth is many metres wrong, and we do not know what the exact wrongness of it is. Therefore we have to leave the metre exactly where it stands. Our own yard measure we have not pretended to do anything of the kind with, but you will hear to-night from the most capable of Englishmen on this subject the great care which we take not to make or invent a new theoretical measure, but to have one exact yard measure in the world from which all our astronomers and navigators may make their calculations with such precision that it shall have no human error if possible mixed up with it, and it is with this invariable yard that we have made all our calculations. If our yard is wrong and not

properly taken care of and any error arises, then all our grand calculations about the distance of the sun and the diameter of the sun and those things are all in error also. But I think you will go from this meeting to-night with the conviction with which I came to it, namely, that our calculations have been made so conscientiously and scrupulously with a standard so carefully observed that we believe you may trust the astronomers who use the English yard measure.

MR. CHISHOLM: In this lecture it is not proposed to treat of weights and measures generally; of those that are in daily use throughout all civilized countries. It is only of the standards of weights and measures that I propose to address you, the material representatives of those primary units by which all commercial weights and measures are regulated.

The subject even thus reduced is still a large one. Many quarto and octavo volumes have been written upon the standard weights and measures of different countries in ancient and modern times. But the time of the lecture is limited, and I shall use my best endeavours to confine my remarks so as to call your attention to the more important points.

2. I propose first to refer to the earliest standards of weights and measures, and to those systems of ancient countries, from which there can be no doubt that all other systems have been derived, with the single exception of the decimal metric system, which is of an original character.

The use of weights and measures must have been one of the earliest necessities of civilized life. Josephus mentions the Jewish tradition that Cain, after his wanderings, built a city in the land of Nod, and was the inventor of weights and measures. The extreme antiquity of the use of weights and measures is also shown by the fact that the ancient heathens attributed the origin of weights and measures to the gods;—the Egyptians to their god Theuth or Thoth, and the Greeks to Mercury.

As regards the original standards of weights and measures, we learn from the most ancient records that the practice was to derive all other measures, as well as weights, from a recognized

standard unit measure of length, and that the cube of this unit, or of a determinate aliquot part or multiple, formed the unit measure of capacity; and the weight of water or other liquid contained in the standard measure of capacity formed the unit of weight.

3. We learn too, not only from ancient records, but also from the very names of the measures of length, that the proportions of the human body were taken to form the scale of measures of length, and that the cubit, or average length from the point of a man's elbow to the extremity of his middle finger, was practically adopted as the most convenient standard unit of length. The following scale of these natural proportions of the human body was most generally recognized, the digit, or breadth of the middle part of the first joint of the forefinger, being the lowest unit of the scale:

The Digit = 1 part.
Palm or handbreadth, = 4 parts.
Span, = 12 parts.
Foot, = 16 parts.
Cubit, = 24 parts.
Arms-length, or step, or single pace, = 40 parts.
Double pace, or stride, = 80 parts.

Fathom, or greatest length of extended arms to the tips of the fingers, equal to the height of a man = 96 parts.

4. The cubit is the only measure of length mentioned in the book of Genesis as in use before the flood. The earliest systems of weights and measures of which we now have any knowledge, were the analogous systems established in Chaldæa, Egypt, and Phœnicia. All these systems were based upon the cubit as the standard unit measure of length, and it is to these systems that the derivation of the weights and measures used in almost all civilized countries can be traced.

Of these early systems, the Egyptian has probably had the greatest influence upon those of other countries. It is generally admitted that the Egyptian weights and measures passed into Asia Minor, and Judæa, as well as into Greece; and with some modifications extended to Italy, where they were adopted by the Romans, and subsequently by all European nations.

5. At the earliest period of Egyptian history, two different cubits appear to have been in use, as computed from the external and internal dimensions of the great pyramid. In other words, one appears to have been used for the measurement of land, the other for measuring buildings. The first of these two measures of length was the common or natural cubit of 6 palms or hand-breadths and 24 digits. This was the cubit of a man mentioned in the earlier books of the Bible. Two-thirds of this cubit formed the ancient Egyptian foot. We have the recorded evidence of the most ancient authors that the length of one of the sides of the square base of the great pyramid was 500 Egyptian cubits or 750 feet. This great pyramid was the first built of all the Egyptian pyramids, and is believed to have been erected more than 4,000 years ago, and during the life time of Noah. We have the evidence of Sir Henry James, the head of the Ordnance Survey Department, that the mean length of the side of the original base of the great pyramid is 760 English feet, according to the most authoritative measurements, the latest being by the Ordnance Surveyors in 1868. The length of the ancient Egyptian foot is consequently shown to be 12·16 English inches, and the cubit 18·24 inches.

This common cubit was identical with the Phœnician or Olympic cubit, afterwards adopted in Greece. We have the testimony of Herodotus that appears to confirm this length of the common Egyptian cubit. Writing about 450 B.C. he says that "the Egyptian cubit is equal to that of Samos," and that has been construed as meaning the Greek cubit. Now the length of the Greek cubit has been satisfactorily ascertained from a recent measurement of the *Hecatompedon* at Athens, so called in Greek, as being the measure of 100 Greek feet. It was the platform on which the Parthenon stood. The ancient Greek foot has thus been found to be also equal to 12·16 inches, and the Greek cubit, one-half more, to 18·24 inches.

This determined length of the ancient natural cubit serves to compute the actual height of two of the old giants mentioned in the early books of the Bible. The dimensions of the iron bed of the giant Og, king of Bashan, are stated to have been 9 cubits

long, and 4 cubits broad, "after the cubit of a man." It was therefore about 13½ English feet long and 6 broad. According to the reckoning of Maimonides, the learned Jew, that a bed was usually one-third longer than the height of a man, Og must have been 9 feet high; and the height of the giant Goliath of Gath, stated in the first book of Samuel to have been 6 cubits and a span, must have been 9 feet 6 inches.

6. The second of the two ancient Egyptian cubits was the royal cubit or cubit of Memphis, of 7 palms, or 28 digits. This cubit was one hands-breadth longer than the common cubit. The origin of a second measure of this increased length and its convenience in measuring are obvious. In measuring, a man would begin by laying down his right forearm, from the point of his elbow to the end of his middle finger; then he would naturally put his left hand down to mark the place, and would continue to measure from the breadth of his hand, thus giving to each cubit an extra hands-breadth or palm. I am informed that a similar mode of measuring muslin and cloth is still practised in India. Exactly the same thing was done in this country, as I shall presently show.

7. In confirmation of the practice of adding a hands-breadth to the cubit, I may refer also to the passage in Ezekiel, describing the vision of an angel with "a measuring reed, 6 cubits long by the cubit and a hands-breadth." All the measurements with this reed were also even cubits. The length of the Egyptian royal or sacred cubit was computed by Sir Isaac Newton from Mr. Greaves' measurements of the internal dimensions of the great pyramid, to be equal to 20·7 English inches. His computation was based on the known fact that even units of measure were used for such buildings, as may be proved by the measurements recorded in the Bible, more particularly those of the Tabernacle built by Moses. But we have other and most conclusive evidence of the length of the Egyptian royal cubit. There is the celebrated nilometer, or stone building in the form of a well, for measuring the rise of the Nile, and marked with 16 cubits. I have here an account of the measurements made in Egypt by the Ordnance Survey Officers, with a photograph of the nilometer, said by

Arabian writers to have been set up by Joseph during his regency in Egypt, about 3600 years ago. There are also in existence no less than ten standard cubit measures of the time of the Pharaohs. Some of these are made of wood, and are in excellent preservation, others are of stone more or less fractured. The antiquity and authenticity of these cubit measures are undoubted, the date of their construction extending back to a period not indeed of the building of the great pyramid, but yet more than 3500 years ago. They all concur in making the royal cubit of 7 palms equal to about 20·7 English inches.

8. Perhaps the most perfect of these standards is the cubit of Amènophis, discovered amongst the ruins of Memphis, early in this century, and secured by M. Drovetti, Consul-General of France in Egypt. This ancient standard cubit is now deposited in the Royal Museum at Turin; and I have here a model of it made by myself, and exhibited here No. 223 *a*.

It bears in hieroglyphics the date of the reign of Horus, who is believed to have been King of Egypt about 1657 B.C. and to have been the 9th Pharaoh of the 18th dynasty. You may see the lines defining the digit, palm, span, foot, and natural cubit, the whole length being marked as the royal cubit.

9. When the Ptolemaic dynasty was founded in Egypt by Ptolemy Lagus, one of the generals of Alexander the Great, he reformed the Egyptian weights and measures, and is said to have introduced a new cubit, the cubit beladi or cubit of the country. This cubit has ever since been in use in Egypt and neighbouring countries, though in the course of time somewhat modified in its length. It was equal to 21·85 English inches, and though a cubit of 6 palms, it was a little longer than the ancient royal cubit of 7 palms. Ptolemy is also said to have introduced the Phileterian foot, as it was termed, from the name of his prime minister. This was afterwards much used as a standard unit of length. It was $\frac{4}{5}$ of the royal Egyptian Cubit of that period, and was equal to 14·17 English inches.

19. The *black* cubit, also extensively used as a standard unit of length in Egypt when under the Arab dominion, was introduced by the Caliph Almamoun, son of the well-known Caliph Haroun

Alraschid, who established a new system of weights and measures throughout his dominions. The black cubit was equal to 21⅛ English inches, and is said to have been taken from the length of the natural cubit of a favourite gigantic black slave of Almamoun. The nilometer scale in the Island of Rohab, opposite Cairo, is attributed to Almamoun, and by a recent measurement, the mean length of several of its cubits was found to be 21·37. The length of this cubit of 7 palms was only a little less than the cubit beladi.

11. Time will not allow me to go on describing the principal standard measures of length of other countries; but you may see in the tables of standards, which I here exhibit to you, the names and comparative length of different standard units of length of many of the principal countries, both ancient and modern, with their equivalents in imperial measure. I may briefly call your attention to some of these standard measures of length.

Tables of standard weights and measures, ancient and modern.

STANDARDS OF LENGTH.

	ANCIENT STANDARDS.	Equivalents in English inches.
Egypt	Common or natural cubit of 6 palms	= 18·24
	Royal cubit or cubit of Memphis of 7 palms	= 20·67
	Cubit Belady of 7 palms	= 21·85
	Black cubit of 7 palms	= 21·34
Judæa	Cubit of the Sanctuary of 8 palms .	= 25·50
	Talmud or Rabbinical cubit of 7 palms	= 21·85
Chaldæa.—Persia.	Hachemic cubit of 8 palms . . .	= 25·20
Ancient Hindoo .	Cubit of 2 spans	= 18·00
Rome	Pace of 5 Feet	= 58·26
	Foot	= 11·65
	MODERN STANDARDS.	
Egypt	Cubit Chariie of 6 palms . . .	= 19·42
	„ Baladi of 7 „ . . .	= 22·94
	„ Hindasah of 8 „ . . .	= 25·83
	„ of the Architects of 9 palms .	= 29·92

	Ancient Standards.	Equivalents in English inches.
China	Chid or foot	= 14·10
Rome	Braccio, or passetto, of 3 Roman palms	= 26·40
Russia	Archine, $\frac{1}{7}$ of Sagene (7 English feet)	= 28·00
Austria	Klafter, of 6 Austrian feet . . .	= 74·66
Prussia	Rhine foot	= 12·36
France	Toise de Peru, of 6 old French feet, or pieds du roi (12·79 English) .	= 76·74
	Metre	= 39·37
Great Britain . .	Yard	= 36·00

	Talent.			Mina.
	Monetary.		Commercial.	
	Equivalents in Imperial Troy oz.		*lb. Avoird.*	*Grains.*
Chaldæan or Babylonian	Silver	1051	—	$\frac{1}{60}$ = 8410
Babylonian, Assyrian, and Phœnician	Royal	1916	131·4	$\frac{1}{60}$ = 15330
Do.	Commercial	—	65·7	$\frac{1}{60}$ = 7665
Later Babylonian (Shekel unit)	Gold	1579	—	$\frac{1}{60}$ = 15158
Egyptian, Jewish, and Olympic	Monetary	1365·75	—	$\frac{1}{50}$ = 13111
Do.	Commercial	—	64·7	$\frac{1}{60}$ = 9062
Later Jewish, $\frac{1}{3}$ of Monetary or Sacred	Civil	—	46·8	$\frac{1}{60}$ = 5463
Great Alexandrian of brass	Commercial	—	93·6	$\frac{1}{60}$ = 10926
Lesser Alexandrian	Silver	682·5	—	$\frac{1}{60}$ = 5463
Greek-Asiatic and Persian	Monetary	1045	—	$\frac{1}{100}$ = 5015
Euboic and Attic	,,	820	—	$\frac{1}{60}$ = 6558
Attic, from Olympic	Commercial	—	64·6	$\frac{1}{60}$ = 9051
Egypto-Roman, based on Great Alexandrian talent	,,	—	93·6	lb. $\frac{1}{125}$ = 5244
Roman $\frac{72}{75}$ of Egypto-Roman	,,	—	100 lb. = 71·6	lb. $\frac{1}{100}$ = 5015
Carthaginian, or Bosphoric (Mina, now Yousdrouman lb. of Constantinople	Monetary	944	—	$\frac{1}{60}$ = 5666
Arabian or Mahometan roth or lb., of 120 Monetary Dirhems	,,	—	—	lb. = 5244
Do. of Almamoun, based on Michtal = $\frac{1}{72}$ Egypto-Roman lb.	,,	1508 (Canthar.)	—	lb. $\frac{1}{100}$ = 7237
Old German, unit, Cologne Marc $\frac{1}{2}$ lb.	,,	—	—	lb. = 7220
Do. Medicinal	Medicinal	—	—	lb. = 5400
Frankish, Old Livre Esterlin, based on Yousdrouman lb., sent by Almamoun to Charlemagne	Monetary	—	—	lb. = 5666
French, based on Poids de Marc	,, & Com.	Unit, Pile de Charlemagne of 50 Marcs. = 590·2	—	lb. = 7554
Anglo-Saxon	Silver	—	—	lb. = 5400
Old English Merchants' lb.	Commercial	—	—	lb. = 6750
Later English Troy	Monetary	—	—	lb. = 5760
Do. Avoirdupois	Commercial	—	—	lb. = 7000
Metric	Mon. & Com.	—	—	kilog = 1543

STANDARD UNITS OF MEASURES OF CAPACITY.

		Imperial gallon.
Egyptian, Lesser Artaba .	. Cube of Olympic foot	= 6·464
„ Royal Artaba . .	„ $\frac{2}{3}$ of Royal Cubit	= 9·440
„ Grand Artaba of Sephad	„ Olympic or Natural Cubit	= 21·818
Hebrew, Ephah, or Bath . .	„ $\frac{2}{3}$ Natural Cubit . .	= 6·468
„ Hin, $\frac{1}{6}$ of Bath .	. (For liquids)	= 1·078
„ Gomer, $\frac{1}{10}$ of Hin	. (For grain)	= 0·646
Syro-Phœnician and Persian, Cafiz or Metretes . .	Cube of $\frac{1}{2}$ Chaldæan Cubit	= 7·186
„ Artaba (for grain) .	. Double the Cafiz	= 14·372
Arab Woebe . . .	. Half „	= 3·593
Greek Metretes (for Liquids)	. Cube of Olympic foot	= 6·464
„ Medimna (for grain)	. $1\frac{1}{2}$ Metretes	= 8·615
Roman Amphora (for liquids) / „ Quadrantal (for grain)	80 lb. weight of wine	= 5·725
„ Modius „	. $\frac{1}{3}$ of Quadrantal	= 1·908
Old Winchester bushel .	. 2·150 cubic inches .	= 7·754
Queen Anne's wine gallon	. 231 „ „ .	= 0·834
Imperial bushel . .	. 80 lb. weight of water, 2·218·191 cub. in. .	= 8·000
Imperial gallon. . .	. 10 lb. weight of water, or 277·274 cub. in. .	= 1·000
Metric Litre (for liquids) .	. Cube of Decimetre	= 0·220
„ Hectolitre (for grain)	. 100 litres . . .	= 22·018

12. The digit has been mentioned as the smallest unit in the scale of natural measures. The inch, or thumbs-breadth was first introduced by the Romans, in accordance with their duodecimal scale. Our word *inch* is derived from the Latin *uncia*, used by the Romans both as the twelfth part of their foot, and as the twelfth part of their pound, whence also our word *ounce*.

13. The old French foot or *pied du roi* is said traditionally to have been the length of Charlemagne's foot, as the English yard has been said to have been the length of Henry the First's arm. Six of these old French feet made the toise, which was made the standard unit of length in France early in the last century, and was used for measuring an arc of the meridian in Peru. The iron standard toise, actually used in this measurement, and known as the *Toise de Peru*, became the legal standard measure in length in France, until the metre was substituted for it. It was also the standard unit of length for all measurement of arcs of the meridian

made in Europe. The standard toise of the Ordnance Survey Department is exhibited here.

You may notice that the English yard is very nearly the same length as double the original cubit. It is very probable that the yard was derived by the Anglo-Saxons from this early standard measure, from being the length of double the natural cubit.

14. We have not equally satisfactory evidence of ancient standard weights. The best authorities agree that both in Egypt and in other early civilized countries, the weight of water contained in the measure of the cube of the standard foot, $\frac{2}{3}$ of the cubit, constituted the larger unit of weight, the talent; and that a determinate aliquot part, the 50th, the 60th, or the 100th part, constituted the lesser unit of weight, the mina. Taking the foot = 12·16 English inches, the measure of the Egyptian cubic foot, the larger unit of capacity, would be equal to 1798 of our cubic inches, or about $\frac{4}{5}$ths of our bushel, and the common talent of the market in Egypt, or Kikkaz, as it was termed, was equal to about 64¾ lb. avoirdupois. The commercial mina, its 50th part, would thus be equal to about 1¼ avoirdupois lb., and the capacity of the mina weight of water, which was the lesser unit of capacity, would be about 36 cubic inches, very nearly equal to an Imperial pint.

15. That the mina was the ancient unit of weight in Egypt, and also the unit for measures of capacity by its weight of their liquid contents, is shown from an ancient inscription on the walls of the great temple at Karnac, recording the victories of Thothmes III., who, according to Sir Gardiner Wilkinson, reigned about 1445 B.C. This inscription is said by Dr. Birch, in his annals of this king, to record the number of mina weights of large quantities of wine, honey, spices, dates, and bitumen, taken as booty in war, or imposed as tribute.

16. As the modern standard unit of weight, the pound (from the latin *Pondus*) was derived from and is the representative of the ancient mina, it may be interesting to examine more closely into the actual weight of this standard unit. The corresponding word *Mana* was used as the Babylonian unit of weight, and is thought to be related to the more modern Arabic word *manah*, to count. The fact is, however, that the weight of the ancient mina

varied in the different countries, and at different periods, just as the weight of the European pound has varied. There was also the same practice in ancient times of a different value being assigned to the same nominal unit of weight, according as it was used for monetary or commercial purposes, as has existed in this country up to the present time, when we have our pound troy and pound avoirdupois.

17. We have certain knowledge of the system of ancient Assyrian and Babylonian weight from the remarkable series of standard weights discovered in the ruins of Nineveh by Mr. Layard and now in the British Museum. There are two series of standard weights, bearing marks of their denominations and periods of construction. The weights of the principal series, 16 in number, are of bronze, in the form of a lion crouching on a pedestal, and bearing cuneiform inscriptions. The second series is of stone or marble, 13 in number, and of a rounded or oval form, representing a duck sitting with the head turned flat on the back. They are all of about the 8th century B.C. But there are also other ancient Babylonian duck weights of stone in the British Museum, believed to be of far earlier date.

In all these ancient weights, two distinct systems are represented, the one double the weight of the other of the same denomination. The mean weight of the mina, the unit of the first series, is 15,330 English grains, and of the second 7,665 grains. The larger series was the royal set, "Manahs of the king," the smaller series the commercial set, "Manahs of the country." In Babylon, the sexagesimal scale was used, as it was by the ancient Chaldæans, from whose observations of the motions of the heavenly bodies we have the circle divided into 360 degrees, and the hour into 60 minutes, with a further sub-division of 60 seconds. The Babylonian talent was 60 mina. The manah was divided into 60 shekels. The mean weight of the heavier Babylonian shekel has been computed to be 266 English grains, and of the lighter shekel 133 grains. In later times, the weight of the Jewish shekel was intermediate between that of the two Babylonian shekels, the average weight of the earliest Jewish silver shekels coined in the time of the Maccabees being about 220 grains.

The talent both of silver and of gold is frequently mentioned in the Holy Scriptures. I may here remind you of the two talents of silver obtained by Gehazi, the servant of Elisha, from Naaman the Syrian, who urged him to accept these two talents, and bound them in two bags, which were laid upon two of Naaman's servants to carry to Gehazi's home. One of these talents of silver was a pretty heavy load, without the change of raiment that accompanied it. It was equal to 92½ of our avoirdupois lbs. The two talents were therefore equal to nearly £700 of our money, even reckoning the value of silver in our own time. According to the Biblical Chronology, Elisha lived about 900 B.C. Up to that period and for some time after, no coined money was in existence and payments were always made in gold and silver by weight. The precise epoch of the introduction of coined money is not known, but no coined gold or silver money is believed to have existed previously to the 7th or 8th century B.C. Long before, however, gold and silver rings of specified weight are stated to have been current. The first king who is known, from the evidence of coins still existing, to have coined gold money was Crœsus, king of Lydia, in Asia Minor, at the beginning of the 6th century B.C.

18. I must not, however, give any more time to the ancient standard weights and measures, and to the standards of foreign countries. It may be sufficient to direct your attention to the table of ancient and modern weights and measures of various countries now exhibited to you, with their equivalent values in Imperial weight and measure.

19. Our English standard units of measures of length and capacity and of weight—the yard, bushel, and pound—have come down to us from the Saxons, though some modifications of the two last-mentioned units have since been made. The earliest recorded standard of length in this country was the yard or *gird* of the Saxon kings kept at Winchester. King Edgar is recorded to have decreed, with the consent of his *Wites* or council, that "the measure of Winchester should be the standard." No change was made by the Normans in the Saxon system of weights and measures established in England, and by a statute of William the Conqueror it was ordained that "the measures and weights should

be true and stamped in all parts of the country, as had before been established by law."

20. The only change that appears to have been made at the conquest was in the custody of the standards, which were transferred from Winchester to the Exchequer at Westminster, where they were placed under the custody of the King's chamberlains. The Exchequer itself was a Norman Institution, and even up to the present time there may be seen at Rouen the ancient building of the Norman Exchequer, with the inscription upon it, "Le père de l'Echiquier de Londres." The English standards of weight and measure were deposited by the King's orders in a consecrated building, just as the standards of old times in ancient countries were placed in their temples. Together with the Royal treasures, they were placed in the Crypt Chapel of Edward the Confessor in the cloisters of Westminster Abbey, since known as the Pyx Chapel. This portion of the old Abbey of Westminster then became vested in the Sovereign, and has ever since been under the custody of the officer who has had charge of the standards. Some of the old standards, which were not required for actual use, remained in the Pyx Chapel nearly up to the present time. The office of the King's chamberlains, which was a part of the old Exchequer Office, was not abolished till 1826, when the custody of the standards was transferred to the auditor of the Exchequer. It was again transferred to the Comptroller General of the Exchequer in 1834, when the old establishment of the Exchequer was remodelled.

In 1866, the Exchequer ceased to be a separate office of the Government, and was amalgamated with the Audit Office. The new office of Warden of the Standards was created in pursuance of recommendations of the Standards Commission; and to this office I was appointed in consequence of my long experience as an exchequer Officer, which began before the Chamberlain's Office was abolished. The Exchequer standards became the Board of Trade standards, the new office being placed under the Board of Trade. These standards include also those of Coinage, the standard Trial plates for testing the gold and silver moneys coined at the Mint, having been entrusted, to-

gether with the standards of weights and measures, to the same officer of the King, certainly from the time of Edward III., and probably from the time of the Conquest. The standard Trial plates were always kept in the Pyx Chapel, the custody and keys of which were thus also transferred to me. But I have preferred to keep the standard Trial plates, with the Imperial standards of weight and measure, in the fire-proof strong room at my office, which has been specially fitted up for their reception. They are kept there in fire-proof iron chests.

21. There is every reason to believe that the standard yard of Henry VII. the Winchester copy of which is exhibited here, represents accurately the length of the old Anglo-Saxon yard. It does not differ from our present standard more than one foot rule differs from another, that is to say, it is about 0.01 inch shorter. You will see that I have placed it on our model of an Inspector's comparing yard, also exhibited here, so that any difference of length can be observed. The yard and the ell were originally the same measure in this country. From the period of the Conquest, down to the reign of Richard II., the statutes and official documents were generally in Latin, and sometimes in Norman French, and the yard and ell (*virga* and *verge*, *ulna* and *aulne*) are used indiscriminately as the same unit of length. In the clause of Magna Charta relating to weights and measures, the term *Ulna* is used as the unit of length for cloth, whilst in Doomsday book, land measured by the yard or yard-land, is called *Terra Virgata*. The old English yard was extended to our later English cloth ell first by making it only one inch longer. By the statute of Henry VIII. the measure of the cloth yard was declared to be a yard and an inch or thumbs-breadth. Having thus got this little addition to the yard, the next process was probably to make a cloth yard of 37 inches, and add a hands-breadth, and by repeating this process to get a cloth ell of 45 inches in Queen Elizabeth's time. I now produce to you Queen Elizabeth's standard yard of 36 inches, and ell of 45 inches, both marked on the same bronze bar.

22. The earliest standard of monetary weight in England was the old pound of the Saxon moneyers in use at the Mint before the Conquest. The only legal standard of this pound, of which any

account has come down to us, was the Mint pound of the Tower of London, known as the Tower pound. It was of the same weight as the old Apothecaries or medicinal lb. of Germany, and was equal to 5400 of our later Troy or Imperial grains. In 1842, an ancient weight of brass was discovered in the Pyx Chapel, that weighed 5409 Troy grains, and was evidently an old monetary pound, somewhat increased in weight from oxidation. This moneyer's pound was the ancient pound sterling of silver, divided into 20 shillings, each into 12 d. or pennyweights. We find that up to the end of the reign of Edward III., the weight of all gold and silver articles in the King's Treasury was expressed in our old Exchequer records in pounds, shillings, and pence, *pois de la Towre*, or *pois d' orfévres*. The pennyweight contained 32 grains, and thus the Tower pound contained 7680 monetary grains, or grains of wheat. The pennyweight was equal to $22\frac{1}{2}$ Troy grains, which is the average weight of the Saxon and Norman coined silver pennies.

The mark was $\frac{2}{3}$ of the Tower pound, and was used for denoting both the weight and value of silver under the Norman sovereigns. It was equal to 3600 Troy grains, and thus did not sensibly differ from the ancient unit of money weight in Germany known as the Cologne Mark.

23. The Tower pound ceased to be the legal mint weight in 1527, by an Ordinance of 18 Henry VIII., enacting that "The "Pound Towre shall be no more used and occupied, but al maner "of golde and sylver shall be wayed by the Pounde Troye, which "maketh xii oz Troye, which excedith the Pounde Towre in "weight iii quarters of the oz." From this time to the present, our system of coinage and of weighing gold and silver has been based on the pound Troy.

Troy weight had, however, been introduced into England long before the reign of Henry VIII., and was certainly in general use there in the early part of Henry V., being mentioned in the Act 2 Henry V. c. 4. In the Exchequer records, the earliest mention of Troy weight is in 1 Henry IV. It is probable that it was brought into England from France during the wars and the English occupation under the Black Prince. Its name has been

considered to have been derived from the French town of Troyes, where a celebrated fair was held. There was certainly a known *Livre de Troyes* and *Marc de Troyes* in the time of Edward the third, and the recorded weight of the Marc de Troye, which was half the Livre, was very nearly $\frac{2}{3}$ of our Troy pound.

24. The use of the systems of weight based on the Tower and the Troy pound was confined to the precious metals and drugs. The earliest standard of commercial weight in England was the merchant's pound, *Libra mercatoria*. It is mentioned in a statute of 54 Henry III., as a pound of 25s. used for all other commodities than the precious metals and medicines. It consequently was equal to one-fourth more than the Tower pound, or to 6750 Troy grains. There was also a merchants' pound weighing about 7200 Troy grains, and thus one-third more than the Tower pound, and one-fourth more than the Troy pound, which was used in England, and still more in Scotland.

25. The merchants' pound, whether of 6750 or 7200 Troy grains, was superseded in England by the avoirdupois pound of 7000 troy grains, which appears to have been in use here as early as the year 1303, as it is recited in a weights and measures statute of that year, the 31 Edward III. It was probably introduced from France, as its name indicates, about the same time as the troy pound. The pound avoirdupois was evidently taken from the old French commercial pound of 16 ounces, used at that time in many parts of France. Our existing Imperial standard pound can be clearly proved to be of the same weight as the standard avoirdupois pound of Edward III., and there are good grounds for believing that no substantial difference has occurred in its standard weight, or in that of the troy pound, since these standards were originally established in this country.

The avoirdupois system of weight was legalised in this country by a statute of Henry VIII., which is a curiosity in its way. It ran thus:—"Beef, pork, mutton and veal shall be sold by weight called *Haver au pois*. No person shall take for a pound of beef or pork above one halfpenny, nor for a pound of mutton or veal above three farthings, and less in those countries where they be sold for less." It is upon evidence of this kind that the theory

is founded of the value or purchasing power of money having diminished about fifteen fold in this country since the conquest.

26. As to our early English standard measures of capacity, it is stated in Magna Charta, "there shall be throughout our realm, one measure of wine, one measure of ale, and one measure of corn." This would seem to imply that there were then three distinct standard measures of capacity. But the only one of which we now have any cognisance is the Winchester corn bushel, of a capacity of about $2150\frac{1}{2}$ cubic inches, together with the Winchester corn gallon of $272\frac{1}{4}$ cubic inches, the later standards of which constructed by Henry VII., still remain in the Standards office. A standard ale gallon of 282 cubic inches was added by Queen Elizabeth, and a standard wine gallon of 231 cubic inches by Queen Anne. But all these standard measures of capacity were abolished in 1824, and the new imperial standard gallon, containing 10 lbs. weight of water, and equal to about $272\frac{1}{4}$ cubic inches, was made the standard of capacity for liquid measures, and the imperial standard bushel of 8 gallons was made the standard for measuring dry commodities.

You have now before you the Winchester copies of some of our earlier standards of weights and measures. The original standards are in the Standards office, and are not allowed to be out of my own custody.

27. As regards the old Scotch standards of weights and measures, the old standard of linear measure was the Scotch ell-wand kept at Edinburgh. It was a little less than $37\frac{1}{16}$ English inches. The Scotch acre was a little more than $1\frac{1}{4}$ English acre.

The old Scotch standard of weight was that termed the French pound troy, and was equal to 7609 of our troy grains, although the French commercial pound of troyes is said to have really weighed 7560 grains. This was the unit of weight by which the stipend in meal to many of the Scotch clergy was paid. There was also a popular system in Scotland generally received as the actual equivalent of the English avoirdupois standard, by which $17\frac{1}{2}$ oz. avoir. were equal to the Scotch troy pound of 16 ounces, making the latter equal to $7656\frac{1}{4}$ of our troy grains. Both of these standard

units were recognized by juries in determining the payments of tithes to the Scotch clergy.

The standard of Scotch measure of capacity for liquids was the Scotch pint kept at Sterling, defined in the Scotch Act of 1618 as containing "3 lb. 7 oz. of French troyes weight of clear running water of the water of Leith." This Scotch pint, according to the Sterling jug, was of the capacity of 103·404 English cubic inches, and 16 Scotch pints=6 English ale gallons. As the imperial pint is of the capacity of $34\frac{2}{3}$ cubic inches, the Scotch jug holds a little less than 3 imperial pints.

The unit of Scotch measure of capacity for dry commodities was the Firlot, the standards of which, for there were two, were kept at Linlithgow. The *wheat* Firlot, used also for peas, beans, salt, etc. contained $21\frac{1}{4}$ Scotch pints, or $2197\frac{1}{3}$ cubic inches, a little more than a Winchester bushel. The *barley* Firlot, used also for malt, fruit, potatoes, etc. contained 31 Scotch pints, or $3205\frac{1}{2}$ cubic inches, nearly equal to $1\frac{1}{2}$ Winchester bushel.

But the dry measure used immemorially and universally in Scotland, even up to the present time, was the *Boll*, containing 4 Firlots, or about 6 Winchester bushels. The contents of the Boll however, varied and still continue to vary all over Scotland, and the term is used much as the *sack* is used in England, its contents being specified in imperial weight or measure.

The act of Union with Scotland passed in 1706, prescribed that the English standards only should be used in Scotland; and the new imperial standards of weights and measures were legalised throughout the United Kingdom of Great Britain and Ireland in 1824, and all others declared illegal. The English standards of weights and measures have been legalised in Ireland since the beginning of the reign of Henry VII.

28. The existing standards of weights and measures are the imperial standard yard of bronze, and the imperial standard pound of platinum, a pound avoirdupois, constructed by the standards commission after the loss by fire at the Houses of Parliament in 1834 of the old standard yard and standard troy pound of brass, which had been constructed under the directions of a Committee in the House of Commons in 1758, and left in the

custody of the clerk of the House. These had been made the legal standards by an act of 1824. I can here show you representatives of them. But the Shuckburgh scale, the property of the Royal Society, was at that time considered the scientific standard of length. The new standards were legalised in an Act of 1854. Four Parliamentary copies of the standards are recited in this Act, as available for replacing the imperial standards if lost or damaged, having been constructed by the standards commission with this object. They are deposited as follows:

Copies No. 1, at the Royal Mint.
No. 2, with the Royal Society.
No. 3, at the Observatory, Greenwich.
No. 4, immured in the New Palace at Westminster.

I have brought with me a copy of each of these two primary standards of length and weight, that you may see how they are constructed, and the method adopted for supporting the standard yard on lever rollers so as to maintain as far as possible a uniform length. The standard bushel and gallon, the units of dry and liquid imperial measures of capacity, are secondary standards derived from the standard of weight.

29. It only remains briefly to refer to the standards of the Metric System of weights and measures, first established in France at the close of the last century, as a new and scientific system based on a natural constant, and with a uniform decimal scale. The formation of this new system, which was imperatively required on account of the intolerably defective state and want of uniformity in the weights and measures in France, was entrusted by the French Government to the French Academy of Sciences. It was based by them on the new unit of length, called a Metre, and purporting to be the ten-millionth part of the meridian quadrant passing through France. For determining the length of this meridian quadrant, the length of a line directly north and south, extending for about 9½ degrees of latitude from Durkirk to Barcelona, was measured with all possible accuracy. The unit of measure employed was the double French toise, and four measures consisting of compensating bars were used, after they had been accurately compared with the old French

Standard, known as the *Toise de Perou*, already referred to, as containing 6 old French feet, each of 12 lines. The result was that the length of the ten-millionth part of the whole of the meridian quadrant was computed from the portion of the meridian measured to be 443·296 lines of the *toise* of the Peru, and the length of the new metre was determined to be 1·949 toise. The new standard metre was constructed of platinum and exactly of the computed length, and was deposited at the Palace of the Archives, at Paris, whence it is known as the Mètre des Archives. In point of fact, it has now been found from later measurements of arcs of the meridian that the computed length of the meridian quadrant was too short by about 1472·5 metres. But it will never be worth while to correct the metre by the ten-millionth part of this difference, amounting to about $\frac{1}{7}$ of a millimetre. One of the original metres is exhibited here, and is now before you.

The units of metric measure of capacity and of weight were derived from the metre. The unit of metric capacity called a *Litre* was defined to be a measure of the capacity of a cubic decimetre, or the cube of the tenth part of the metre. The unit of metric weight was defined to be the weight of distilled water contained in a litre, and was called a *kilogram*. For determining the capacity of the litre, an experimental vessel was constructed in the form of a cylinder with the height equal to the diameter. Its volume was intended to be made nearly $2\frac{1}{2}$ that of a cubic decimetre, and its exact volume was determined by accurately measuring its external dimensions. It was then weighed in air against old French weights, and again when immersed in distilled water; the weight of water displaced by it was thus ascertained, and from it was computed the exact weight of water displaced by the volume of a cubic decimetre; this weight was 18827·15 grains of the Poids de Marc, which contained 9216 such grains. A vessel containing this weight of water constituted the litre, the standard of metric capacity, and a standard kilogram of platinum was constructed of this exact weight, and was deposited also at the Palace of the Archives, being known as the *kilogramme des archives*. From these two primary standards of metric length and weight, all other metric measures and weights have since been derived. No standard of

the litre was made, as its true contents could be better ascertained by weighing its contents of water against a standard kilogram.

30. I would here draw your attention to the effects of difference of temperature upon the length of standard bars of linear measure, and to the combined effects of differences of temperature and of weight of air upon the weighings of standard weights in air, and upon the capacity of standard measures as determined by weighing their contents of water in air.

No consideration was given to the effect of these influences upon standards in ancient times, when indeed all the standards were constructed without regard to scientific accuracy. But it has not been so in more recent times, when for determining their true value it has became absolutely necessary to fix the standard temperature of every standard of length, as well as the conditions to which the standards of weight and capacity are to be referred.

As every metallic bar varies more or less in length from changes of its temperature, it is necessary to determine the temperature at which its true length is to be taken, which is called its standard temperature. The standard temperature of the Imperial yard is 62° Fahrenheit. That of the old French standard *toise* was 13° Reaumur, equal to $61\frac{1}{4}$° F. These standards were therefore nearly always of their true length in the ordinary temperature of a comparing room; though when special accuracy was required, it became necessary to determine their temperature by a standard thermometer, and to make allowance for any difference of length caused by their expansion or contraction from their true length at the standard temperature. On the other hand, the French men of science, in order to avoid any difficulties of determining the temperature by the use of a thermometer, and also for the purpose of obtaining the advantage of readily bringing the standard bar to a constant temperature, adopted the temperature of melting ice, 32° Fahr. or 0° Centigrade, as the standard temperature of the metre. This plan, however, has also its disadvantages, as any metallic or even wooden metre which may be of the true length of a metre at 32° Fahr. is sensibly longer at a mean temperature of 62° Fahr. In a scientific comparison also of a French metre with an English yard, the exact temperature of each bar must be

ascertained as well as their rate of expansion, and the true length of each computed at its standard temperature. To give you some idea of the amount of this allowance, I must refer you to this table of the average actual expansion of a metre bar and a yard bar, constructed of materials differing in their rate of expansion, warning you at the same time that it is in each case only an average amount of expansion, as bars even of similar metal and from the same casting are found to differ sensibly in their rate of expansion.

Table of Actual Expansion of a Yard and Metre.

	Of a yard bar.		Of a metre bar.	
	For 1° F.	For 30° F.	For 1° F.	For 30° F.
	in.	in.	mm.	mm.
Of Platinum	0·000171	0·00514	0·00476	0·14280
„ Brass	0·000344	0·01032	0·00956	0·28698
„ Bronze	0·000341	0·01023	0·00947	0·28410
„ Copper	0·000314	0·00942	0·00873	0·26180
„ Wrought Iron . . .	0·000220	0·00660	0·00550	0·16500
„ Cast Iron	0·000198	0·00594	0·00611	0·18333
„ Cast Steel . . .	0·000207	0·00629	0·00575	0·17250
„ Glass	0·000177	0·00532	0·00492	0·14766
„ Pinewood . . .	0·000099	0·00297	0·00275	0·08265

The actual amounts of expansion here stated, even for 30° Fahr. from 32° to 62° may appear to you very small, but they are really considerable when viewed through microscopes, which is the ordinary practice in scientific comparisons of standard bars. You will understand this when I state to you that all the 74 Imperial standard yards constructed by the commission for restoring the standards are marked with the temperature in *hundredths* of a degree of Fahrenheit at which they are of the true length of a yard. Only the Imperial standard itself and one other bronze yard are marked with 62°·00 F. The others vary more or less

from a minimum temperature of 61°·14 to a maximum temperature of 63°·55, an exceptional high temperature of a Swedish iron standard yard.

As to the actual effect of variations in temperature which affect the volume of a standard weight when weighed in air, and consequently the volume of air displaced by it, and of variations of temperature, of barometric pressure, and other conditions of the air, which affect the weight of the volume of air displaced, their effects on the results of the weights in air have to be ascertained by elaborate computations, and allowed for, before the constant and true value of a standard weight, which is its weight in a vacuum, can be determined. I have gone more fully upon this part of the subject in my previous lecture on Balances, to which I may refer you. The results of weighing the contents of water in standard measures of capacity, against standard weights in air, must in like manner be corrected, but time will not allow me now to allude further to this scientific process.

31. So far as regards the conditions of the primary units of weight and capacity of the Imperial and the Metric Systems, which alone may now be referred to, the standard unit of weight in each case is, as I have already stated, made of platinum, and has its true weight in a vacuum. But for all ordinary commercial purposes, a second standard of weight is established, which is of brass, and equal to the primary standard in a vacuum. But its commercial value is its weight in ordinary air, or more strictly in what is considered as the average condition of air in comparing rooms, when the thermometer is at 62° F. and the barometer at 30 inches. This is our standard air; and that of the French is very nearly the same. I now produce before you specimens of the primary and of the commercial standard units of weights of the two systems, the pound and the kilogram, which are amongst the instruments of the Standards Department.

32. There is not the same uniformity in the mode of dealing with the standard units of capacity of the two systems. The English Gallon has its true value when it contains pure water, ten times the weight of the brass commercial standard pound when weighed in standard air. But the French Litre has its true value

when it contains pure water at its maximum density, or about 39° Fahr., of the weight of the brass commercial standard kilogram at 32° F. when weighed in a vacuum. As it is impossible actually to weigh them under such conditions, the practice is to weigh them in air, and then to compute the true value. Like the metre, therefore, the commercial value of the litre in ordinary air does not quite agree with its legal value. The difference is small, and for all commercial purposes may be entirely disregarded. Still it is appreciable, especially in large multiples of the litre. The legal litre contains 1000 grammes weight of water in a vacuum, whilst the commercial litre contains a little less than 999 grammes weight of water in air.

33. I need hardly remind you that the standards of weights and measures are made practically useful by means of duly verified copies of these standards and of their authorized multiples and parts, which are placed in the charge of local officers, whose duty it is to compare all commercial weights and measures with them, and to stamp them when found correct. In all foreign countries, it is the practice for the Government to supply the local officers with the requisite number of verified copies of the standards at the public expense; and this was also the practice in this country, until the imperial standards were established in 1824, when the local authorities were required to furnish at their own cost copies of the standards to their officers, which were to be sent to the Government office for verification. No legal provision was made for reverifying these local standards until 1859, when the weights were required to be reverified at least every five years, and the measures every ten years. Practically they are now reverified much oftener, and many of the weights every year. This constitutes the administrative business of the Standards Department, as distinct from its scientific duties, as it is also charged with all comparisons with the standards required for scientific research. In Great Britain there are upwards of 700 local inspectors of weights and measures who have charge of as many sets of local standards; and there are also more than 600 local inspectors in Ireland, but these use iron sub-standard weights which are locally verified. The inspection of weights and measures is distinct from that of gas

meters. There are also upwards of fifty inspectors of gas meters in Great Britain, and two in Ireland. All of these are furnished with verified copies of the Standard Gas Measures.

34. In conclusion, without referring further to the relative advantages of the Imperial and the Metric System of Weights and Measures, I may briefly state that the metric system has now been adopted in every country of Europe, except Russia and this country. It has also been adopted in Turkey and in most of the States of South America. In the United States of North America, the weights and measures are still regulated by our English standards which were in force during the last century, but preparations are being made for adopting the Metric System. It is thus truly the International System of Weights and Measures, and in order that uniform standards of the Metric System should be furnished to all civilized countries, an International Commission was convoked in Paris in 1870, formed of competent scientific representatives from more than thirty of the principal governments, including that of this country, for the purpose of constructing a sufficient number of New Metric Standards with the highest possible scientific accuracy. These standards are now nearly completed as regards their construction, but a considerable time will probably elapse before their definite verification and their distribution is accomplished.

The CHAIRMAN: I am sure you will unite with me in returning our very grateful thanks to Mr. Chisholm for the admirable manner in which he has condensed an enormous quantity of information, beginning from the earliest periods of civilization and bringing us down to the most recent refinements of his own office. I am sure you have heard it with great interest, and I am sure you all go away as I do, astounded that there should have been such an enormous quantity of information in the world on a subject of great interest, and that you and I should have been ignorant of so much of it. The lecture possessed one great charm which I always find the lectures of the ablest men in each department possess, namely, that the able man puts ten times more into an hour's speaking than the other man puts into a great many hours;

and you have had the whole work of a lifetime, and the whole history of mankind with reference to weights and measures put before you in an interesting and to me in a fascinating manner. At the same time that we must return our most grateful thanks for this lecture, allow me just to say that I think we have another task, or pleasure, or duty, equally incumbent upon us. This is the 29th of a series of free lectures by eminent men of science on profound and difficult subjects which have been given to successive audiences in this room, who have shown similar attention and interest to that which you have shown on this occasion, and who have felt themselves as much indebted as we feel ourselves to-night to those lecturers who have so kindly volunteered their great ability and valuable time, not for our entertainment merely, but for our profound instruction, and I trust real improvement. I hold in my hand a list of 29 such lectures which have been given upon these occasions; and when I combine the institution of these free science lectures with the great exhibition of scientific instruments collected from all the countries of Europe, and also collected historically so as to show the old regions of science as well as its more recent triumphs and progress—I say, when in such circumstances we have a series of lectures delivered by the most distinguished men of England, each in his department, we must feel that this is the opening, as I venture to say, of a new era in the public institutions, may I say in the popular institutions of this kingdom; for I think you will all agree with me that the South Kensington Museum of Art has become a popular and a favourite institution, not only in this city but in the land in general, and that we Englishmen are all proud of ourselves, of our countrymen, and of our country when we walk through that admirable collection which is open to all the world for the refinement of their taste, for their instruction in the most exquisite works of art which have ever proceeded from the brains of human beings. Now I think we owe a great debt of gratitude to those gentlemen who have initiated a new department of a similar description not for matters of art but for matters of science, and who have collected together here for the first time such a number of the marvels of human ingenuity, and of the creations of modern scientific

men by the discoveries which have enabled them to achieve these triumphs of skill. Allow me to say that the collection which has been made here is a collection in the departments with which I happen to be acquainted of most marvellous scientific value, and in many respects of most powerful instructive value. The rooms downstairs are filled with a multitude of apparatus which to most minds achieve things utterly impossible. I do not know if you have noticed that you have a simple set of wheels downstairs to which you communicate the information that you want to know what calculations should be made for the tides in any part of the world that you choose to name. You give to this machine the latitude and longitude of the particular place you want to calculate; the machine grinds round, and on a sheet of paper it gives you the whole calculations of the tides for the place you want. Now the tides, you remember, are created by the attraction of the sun and the attraction of the moon; they are rendered different in each part of the world by the longitude and latitude of that part of the world, and all this calculation is done for you, and handed to you on a piece of paper that you place there to receive them. I only select that as one of the instruments you see downstairs. In the same manner, if you wish to draw some exquisite curve, you may call in an astonishing artist, you may call in a wonderful mathematician, but I would defy any human being with any pencil to create a single curve of the exquisite beauty and the incredible precision of those curves which are manufactured by some of the intelligent little self-acting engines of which you will see specimens downstairs. And allow me to suggest to you what an education it would be for the young men who are rising up to succeed you and me in the duties of life, to come on their holidays and afternoons now and then and see in every department of science what is the cleverest thing that humanity has been able to achieve up to this moment. Would it not give them a fresh start in life; would it not give them an ambition and an aim in life, and would not it be the most admirable instrument of education you could conceive, when they should come and witness these triumphs themselves instead of seeing them described in books, drawn in pictures, or scored out on

black boards. It appears to me then that if this institution were to become permanent in a large building to be erected for the purpose, where we might bring together large diagrams, shewing us all the wonderful discoveries which have been made of remarkable phenomena made plain to our eyes, and see wonderful rooms filled with the developments of all the amazing beauty of colours which the recent discoveries in optics have manifested to the human mind,—colours, the complication and combination and brilliancy and relations of forms of which infinitely transcend anything the human imagination has ever been able to conceive; if you were then to walk through these halls and see what is the most perfect instrument that has been invented by humanity for any given purpose, you would not only receive general instruction, but I am sure that if you saw something in your own profession you would then and there have the ambition to do something as good as that and even perhaps better.

Now, gentlemen, we are indebted for the initiative of such a theoretical elysium of science as I am now suggesting to you to certain official gentlemen whom I wish you to unite along with the lecturer in passing a vote of thanks to. I assure you I find on examination that some four or five men have done all this for you; and those men have laboured and laboured and taken such pains and toil as you can hardly conceive, to initiate this movement and to put it before you as you now see it. In the beginning of their work I know that they encountered such obstacles that they almost gave it up in despair, only there are some Englishmen who never will be prevailed upon to give up anything in despair when they think it is a great work for the public, and when they feel that it may be their duty to carry it out successfully. Now these gentlemen, I happen to know, had to ransack England almost personally before they could get anybody to help them, and then having only got a little help in England they had to seek help abroad; and in order to obtain that they had to get and solicit the different civilised nations of Europe to come and help to instruct you English citizens by shewing you in every department of science what they had been able to do. At last they succeeded. You must be astonished, as I was, to see the wonderful collection

which the German people have sent here of their educational apparatus and of their mechanical apparatus of every kind; and you will be pleased to know that it was with the entire concurrence of the German Government, and, I believe, by the powerful influence of some of the most elevated people in Germany, that a vote was given that they should cordially assist England in this great attempt of hers to educate her rising generation to enable them to perform great achievements in science and its applications. I think it was a kindly act on their part. We have received also from several other nations, Belgium, France, Italy, and many nations, valuable contributions of the same kind. I think it would be a great pity that such a collection should be dispersed. I think it would be a great pity that this should be the last lecture of this kind at which you and I are to have the pleasure of meeting each other. I think it would be an oversight to let this occasion go over without saying that we believe from the wonderful success which has attended these free lectures, and from the wonderful popularity which has been achieved by the lectures of profound and eminent men—as distinguished from mere professional lectures—profound and eminent men of science coming here and endeavouring to make plain to you the profoundest truths of their own special research,—I say it is a new future and a future which deserves very great approbation on our part, and let us hope that this is therefore the nucleus of a new museum to be entrusted I hope to the same able hands which have brought this together, to be collected in larger halls with much greater opportunities for shewing the treasures of science than even this collection gives us, and that the future courses of lectures to be given to the English people in these halls on such occasions will be lectures not only by 25 or 29 of the most distinguished men of science, but that during a considerable portion of the year a regular course of such lectures shall be given by the most distinguished men on the most important subjects, and shall draw a large collection of young men and some of us men not quite so young, to these halls of science. In order to that end, then, giving our best thanks for the past services, and

giving them our warmest wishes that we may enjoy in the future similar and even greater services, I would return our thanks now to Mr. Chisholm and also to those other gentlemen to whom I have already referred, and I would further return our thanks to all the gentlemen who have contributed Free Lectures, and to all those who have assisted them so liberally in bringing this collection together, and say that the English people, as represented by us, are not ungrateful for the very great amount of trouble and pains which have been thus expended for their intellectual, and, I trust, also for their moral good.

THE END.

PRINTED BY TAYLOR AND CO.,
LITTLE QUEEN STREET, LINCOLN'S INN FIELDS

00039804

Digitized with financial assistance from the
Government of Maharashtra
on 18 July, 2018